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SCIENCE

16 June 1961

Vol. 133, No. 3468

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

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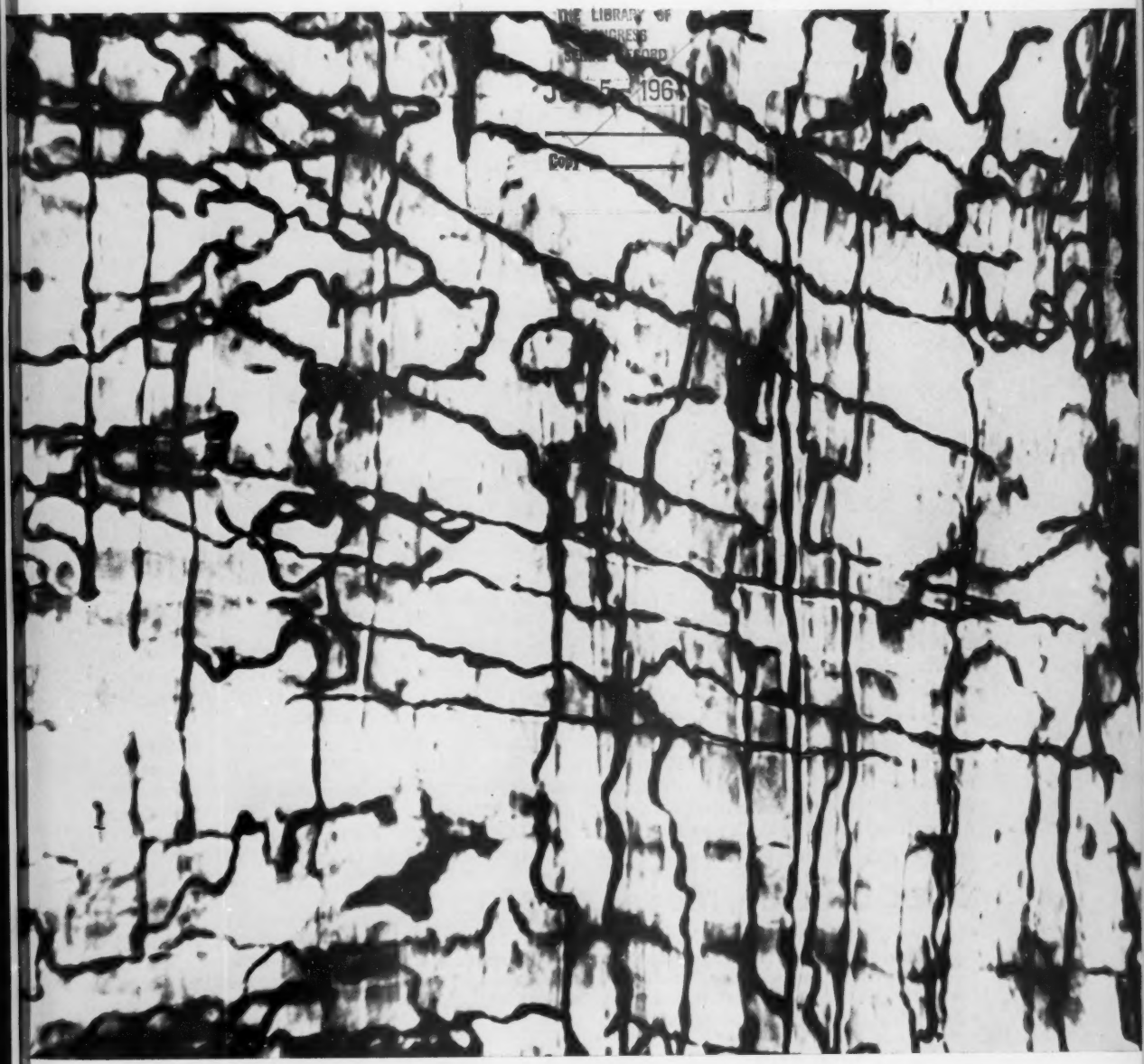

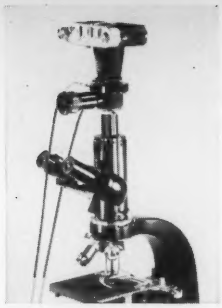

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16 June 1961, Volume 133, Number 3468

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Dilemma of Academic Biology in Europe: *W. V. Consolazio* ... 1892

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The Humoral Regulation of Breathing: *B. W. Armstrong* et al. ... 1897

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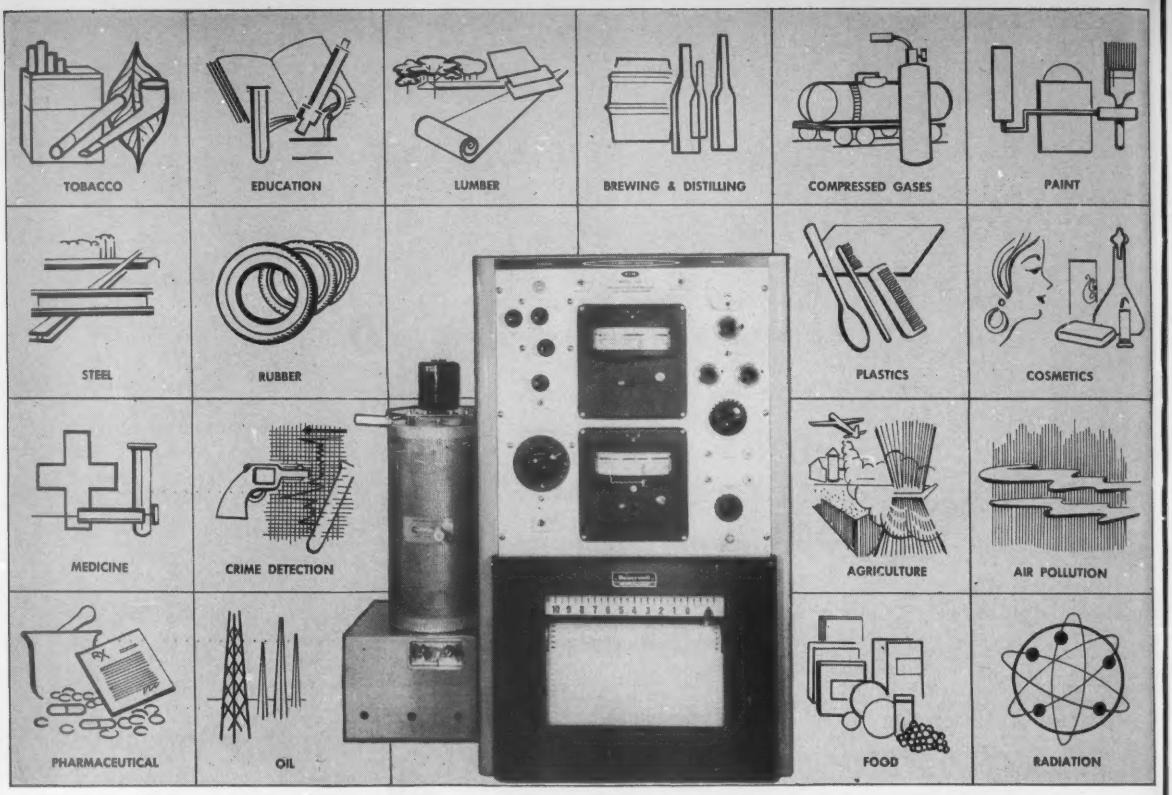
Cover

Capillary network at the juncture between vertical and longitudinal muscle bundles in the tongue of the rat. The blood vessels are revealed by their specific reactivity for alkaline phosphatase (× 420). [Richard A. Ellis, Brown University]



For Research and Development in all Industries

MODEL 500 LINEAR PROGRAMMED HIGH TEMPERATURE GAS CHROMATOGRAPH

The technique of linear programmed temperature gas chromatography combined with high temperature operation makes possible these previously difficult analyses: (1) wide boiling mixtures; (2) gases (H_2, O_2, N_2, CO, CO_2 and light hydrocarbons); (3) trace quantities of high boiling residues in low boiling solvents; (4) correlation of retention times with analytical distillation data; and (5) temperature sensitive materials.

Two such analyses are shown below. Note that with programming, both separations required only one column. Without programming, the methyl ester separation would have required a fixed high column temperature with a loss in resolution of the lower boiling components. In the separation of gases at a constant column temperature, ethane, carbon dioxide and ethylene would not have eluted. To chromatograph this mixture at constant temperature, two columns and two samples are necessary.

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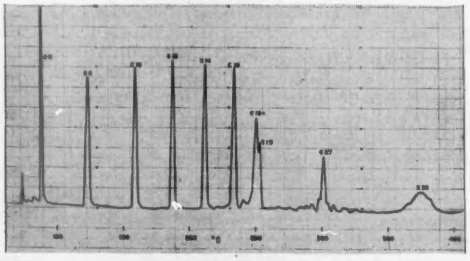
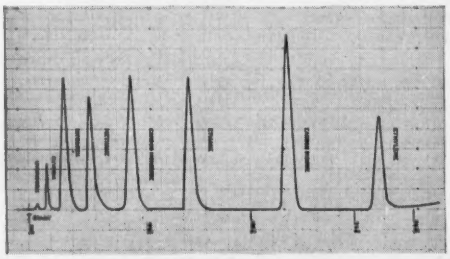


CONDITIONS:
COLUMN TYPE: 2 FT. SILICONE GUM RUBBER
COLUMN TEMP. PROGRAMMED AT 5.6°C/MIN. FROM 75°C to 400°C.
This is a typical programmed temperature chromatogram demonstrating a separation of methyl esters with widely differing boiling points. Observe the sharp peaks, shaped nearly alike.



CONDITIONS:
COLUMN TYPE: 4 FT. MOLECULAR SIEVE 5A
COLUMN TEMP. PROGRAMMED AT 4.0°C/MIN. FROM 35°C TO 247°C.
By programming column temperature, H_2, N_2, O_2, CO, CO_2, ethane, and ethylene all separate on a single column in one run. Using the same column, the programmed temperature technique works equally well for the separation of nitrous and nitric oxide in the presence of these gases.

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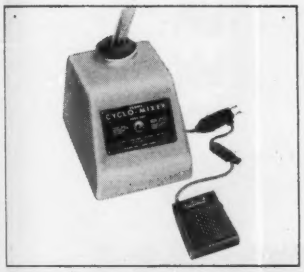

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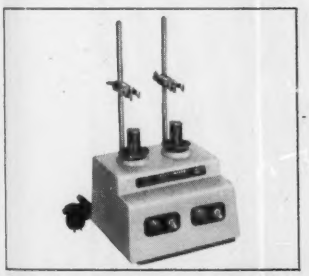

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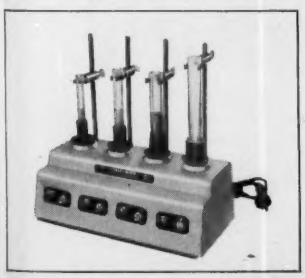

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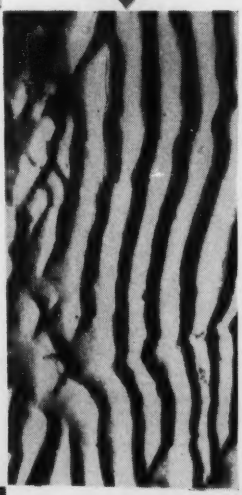

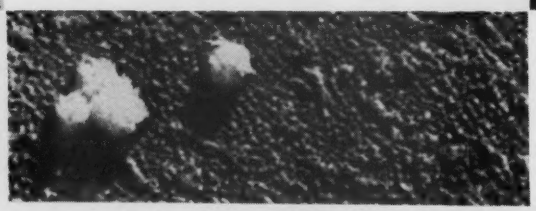

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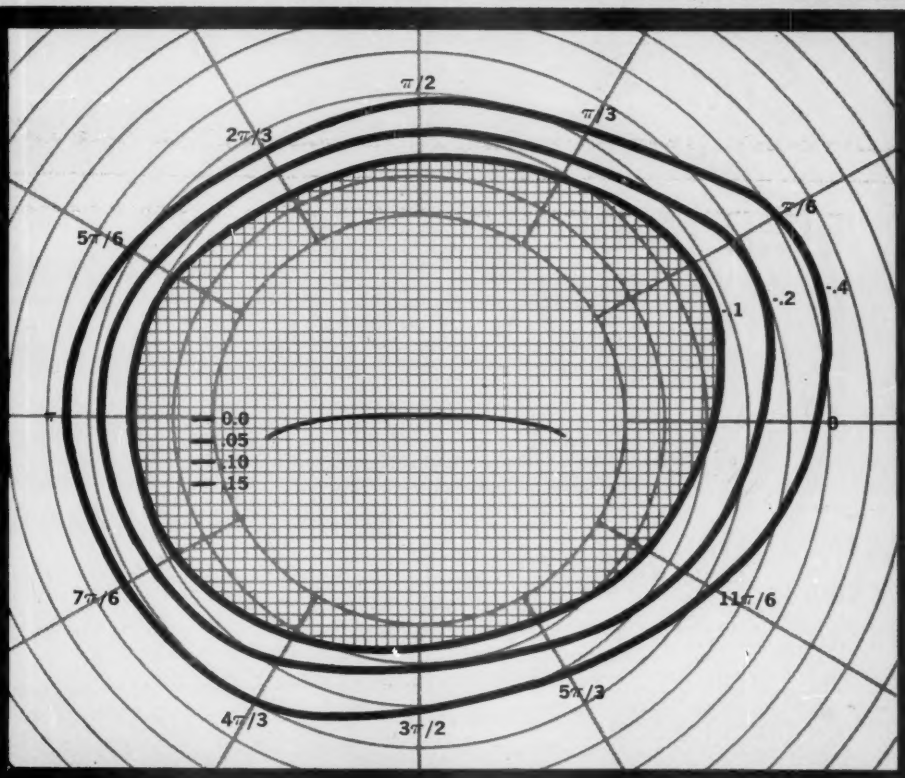

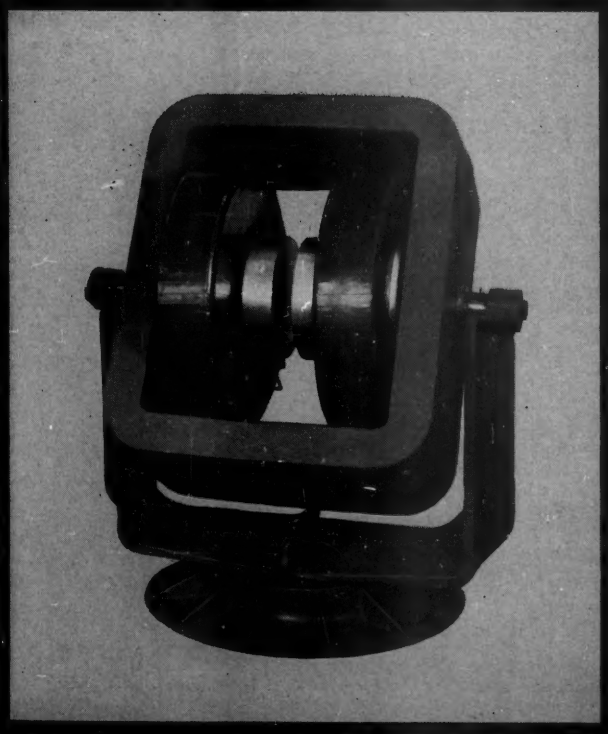

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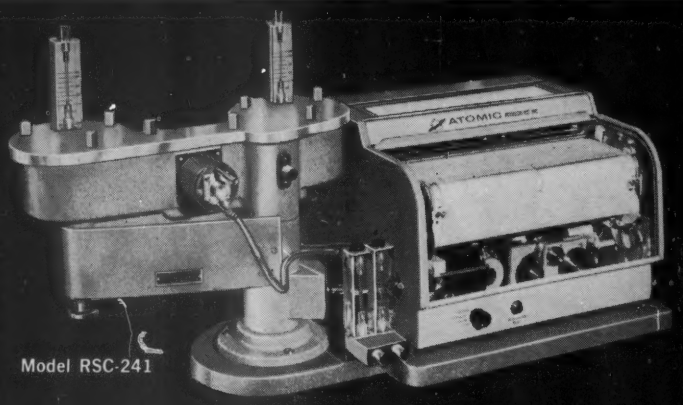

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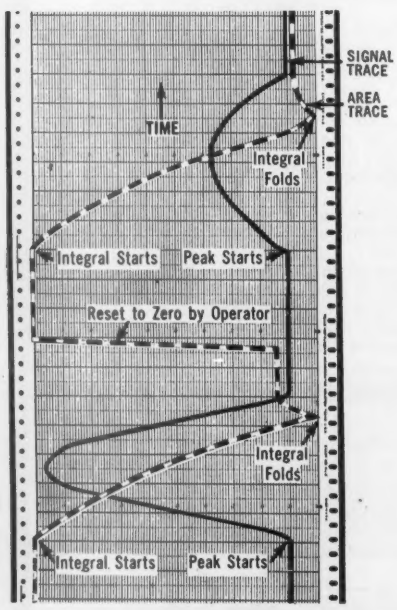

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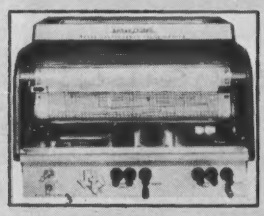

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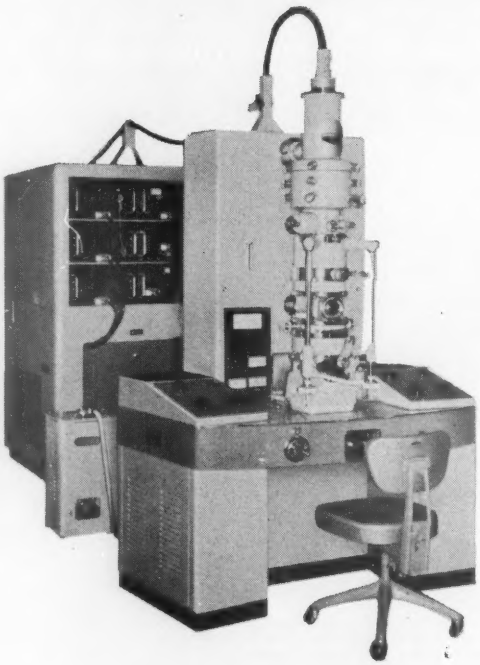

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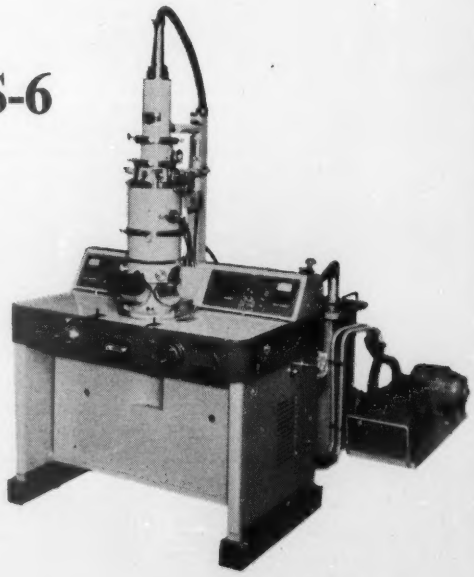

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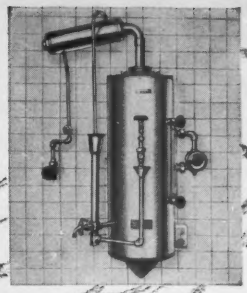

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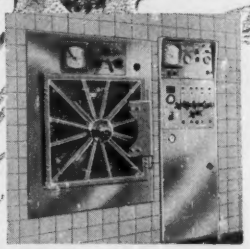

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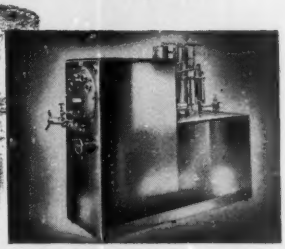

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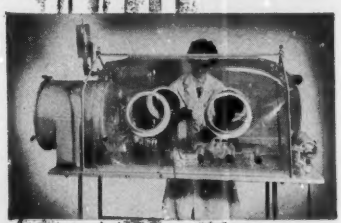

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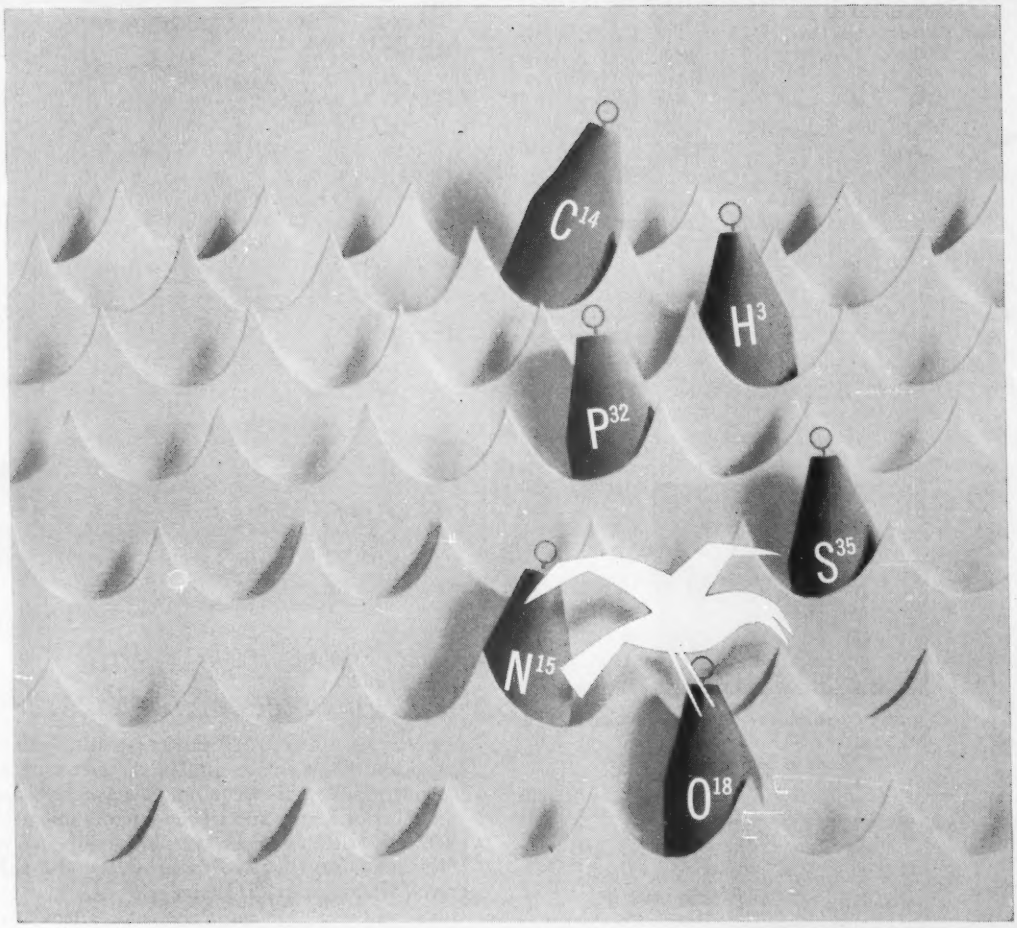

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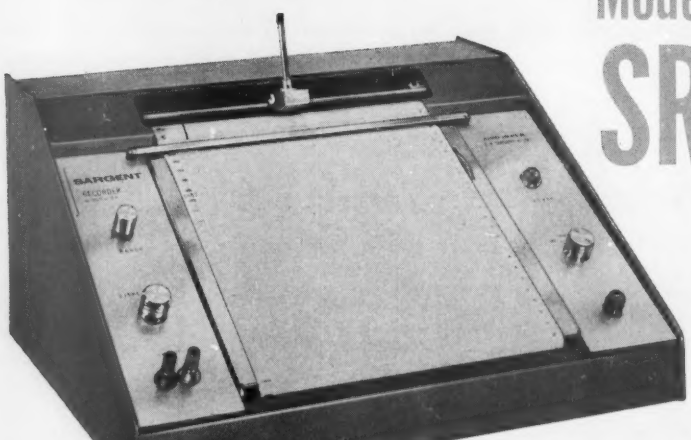

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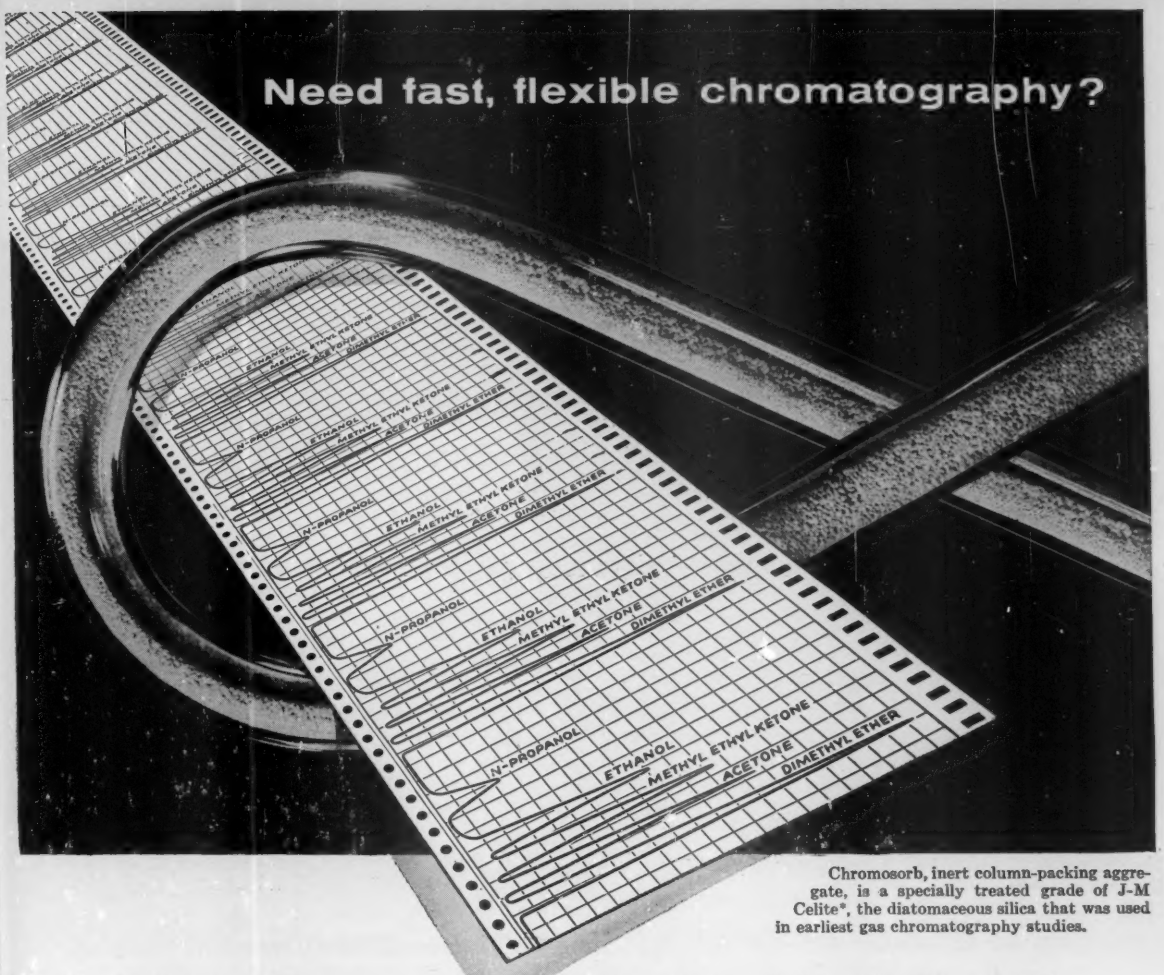

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pH (avg.)	6-7
Surface Area—sq. m./gm. (avg.)	3-5

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Specific Gravity—true	2.30
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Moisture—% by weight, maximum	1.0
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Surface Area (BET Method)—sq. m./gr. (avg.)	3-4

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READERS' SERVICE 16 June 1961

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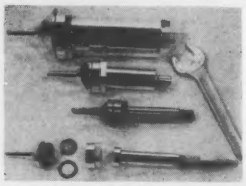

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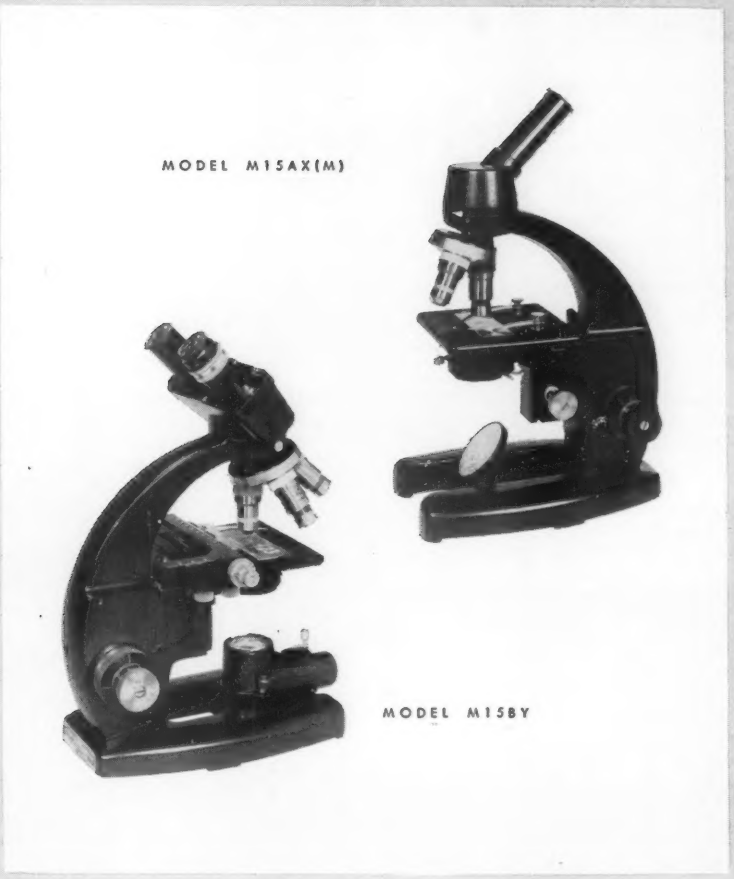

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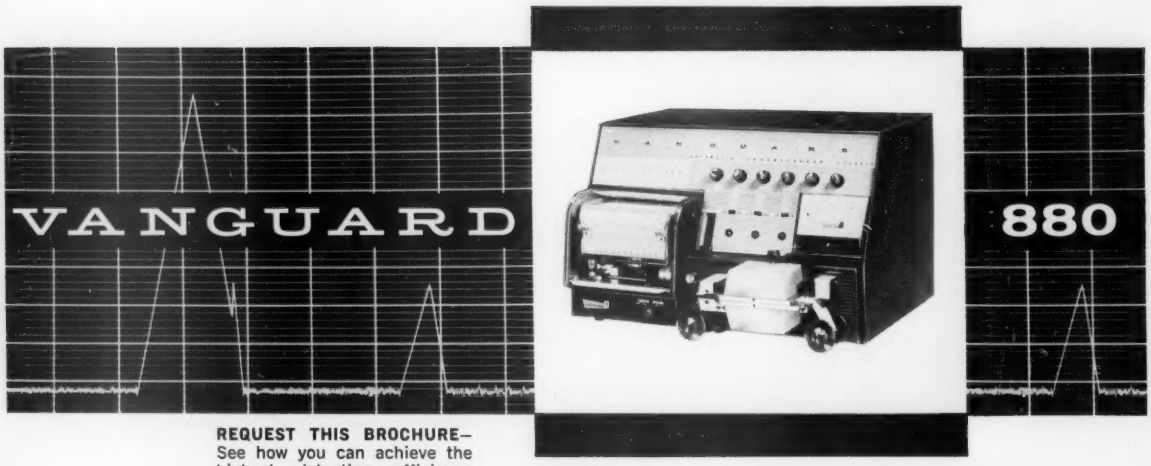
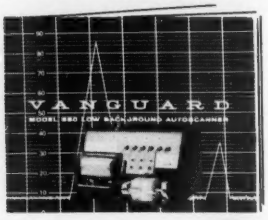


REQUEST THIS BROCHURE—See how you can achieve the highest detection efficiency available for chromatogram scanning. Booklet outlines distinctive features and lists all operational characteristics of the Model 880 AUTOSCANNER.



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A CONTROLLED-ENVIRONMENT INCUBATOR-SHAKER

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For the Growth of Microorganisms under Controlled Conditions of Temperature, Atmosphere, and Agitation

THE PSYCROTHERM is a rigidly controlled environmental incubator with a continuous-duty shaking mechanism. Though it occupies comparatively little floor area, it has 10½ cubic feet of *usable* work space in the incubation chamber, where static and shake cultures can be incubated simultaneously or separately.

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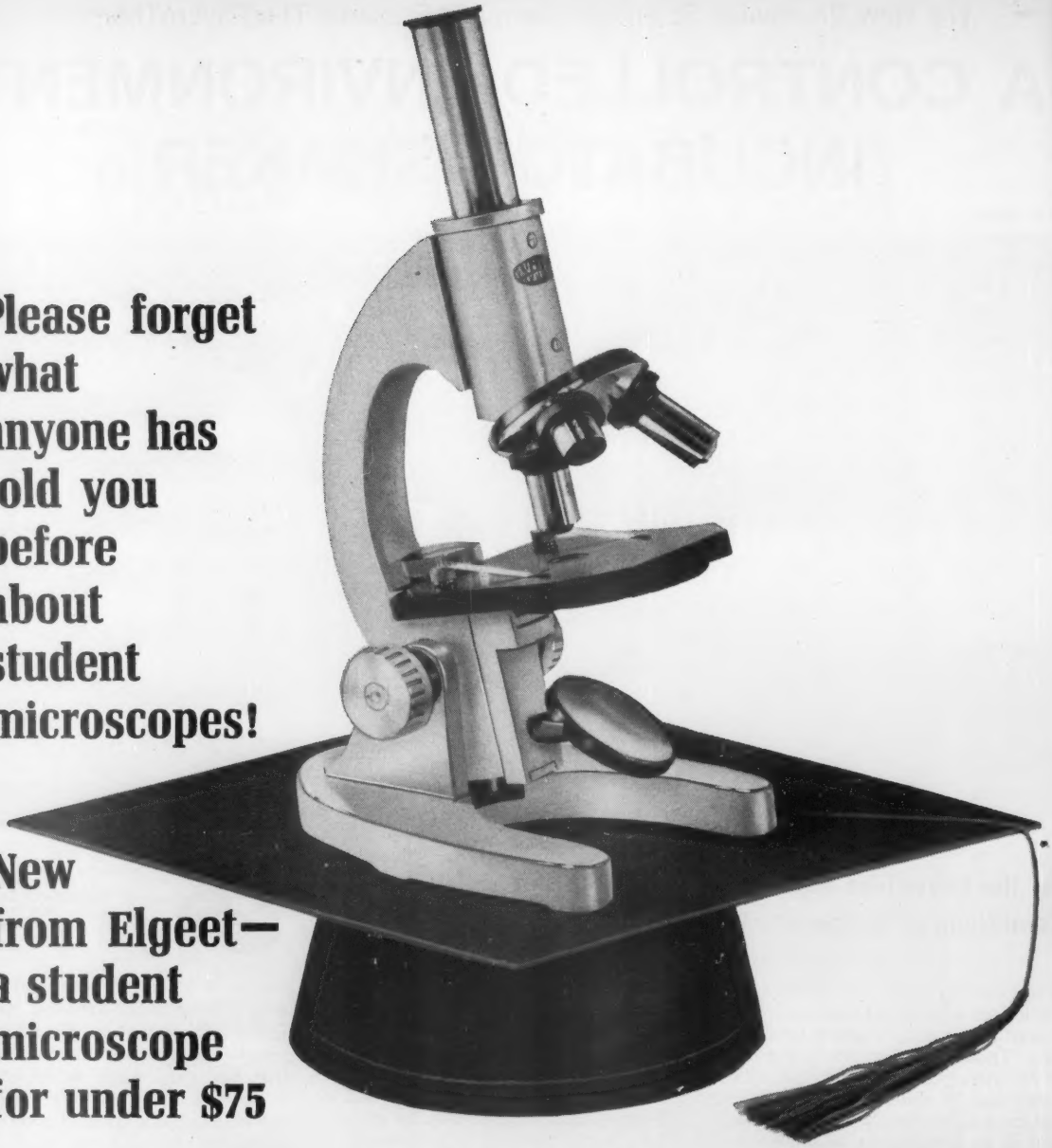

New from Elgeet— a student microscope for under $75

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OBJECTIVES—Hard-coated, parfocal and achromatic.
SS-2—10X and 40X; SS-3—4X, 10X and 40X.
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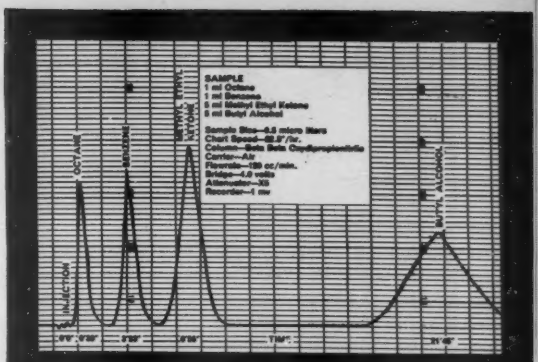

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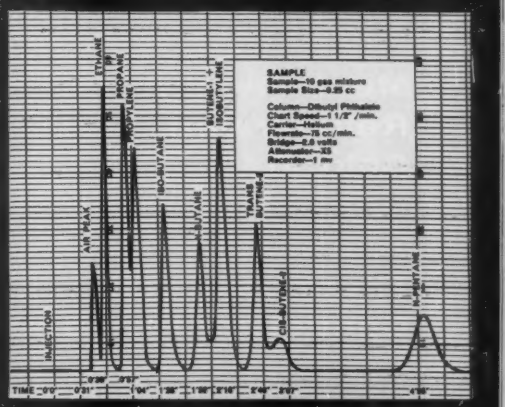

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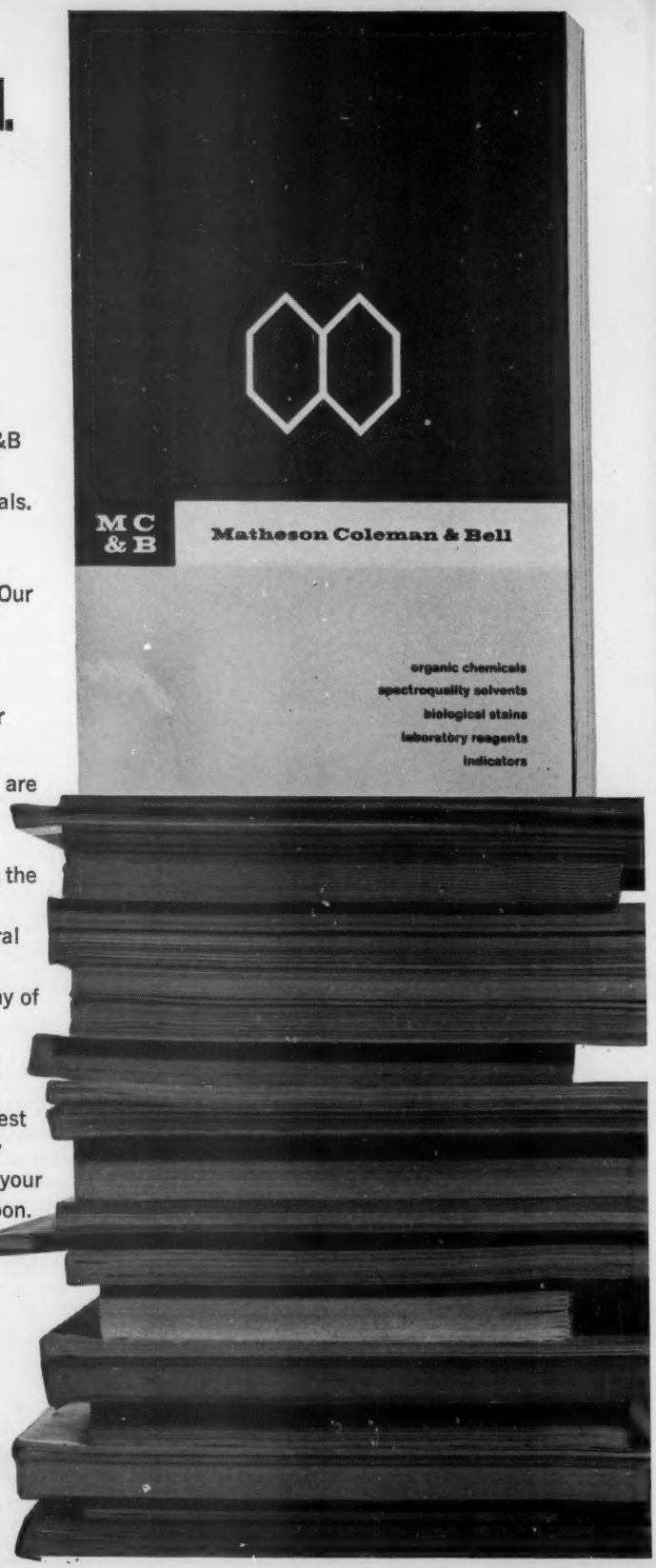

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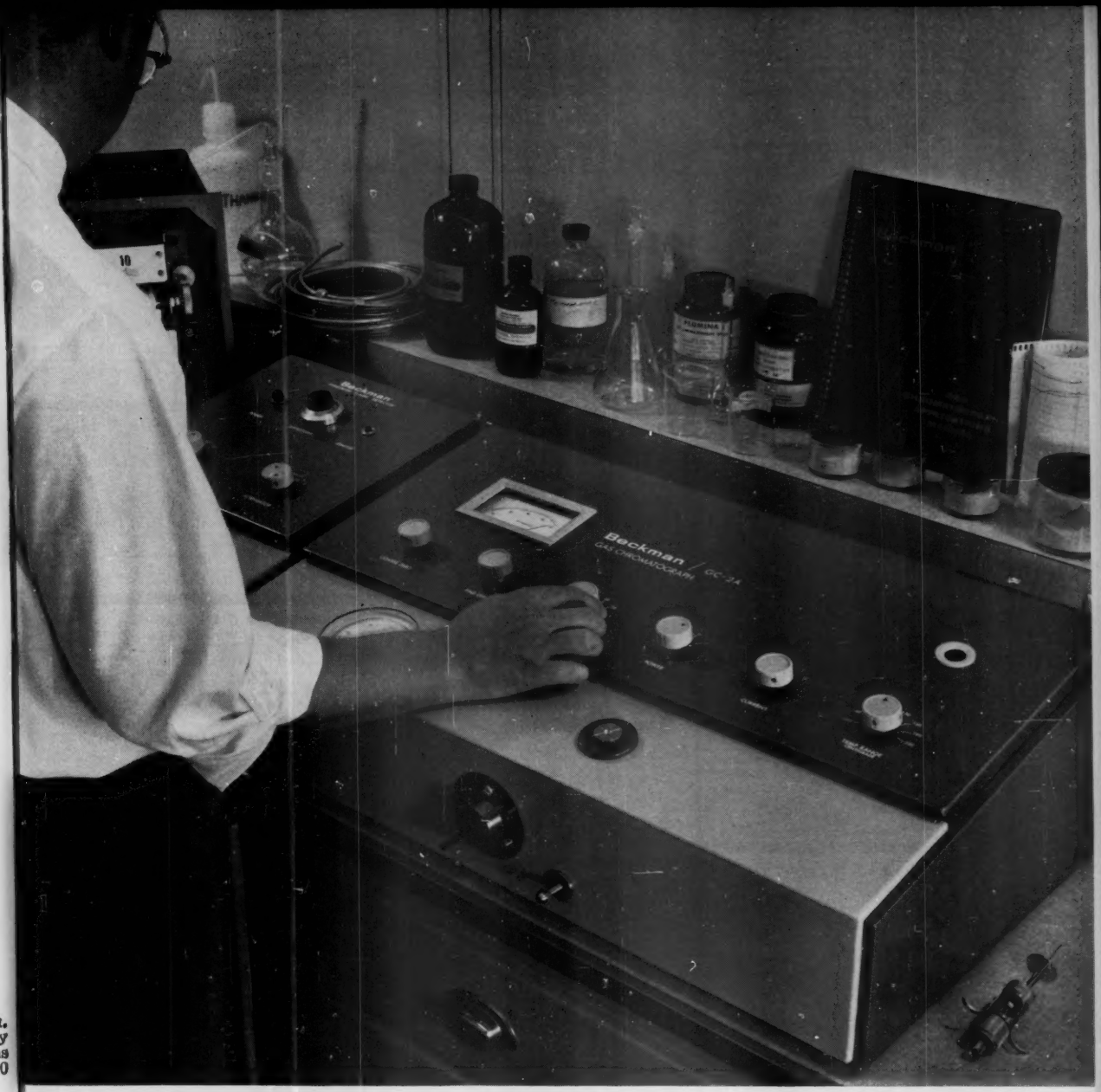

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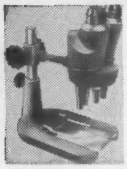

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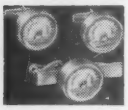

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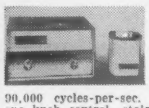

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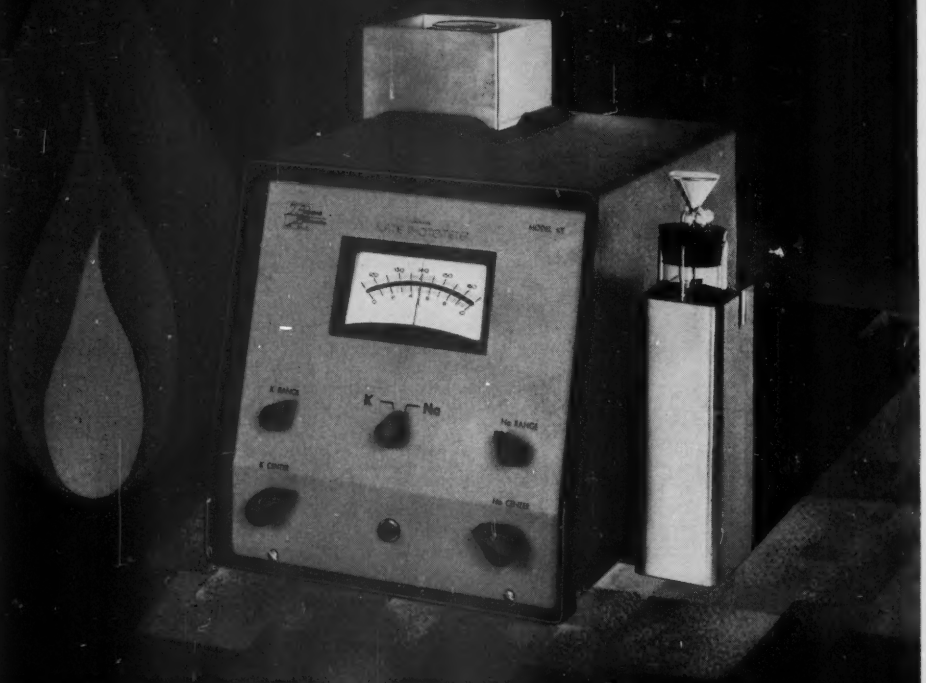

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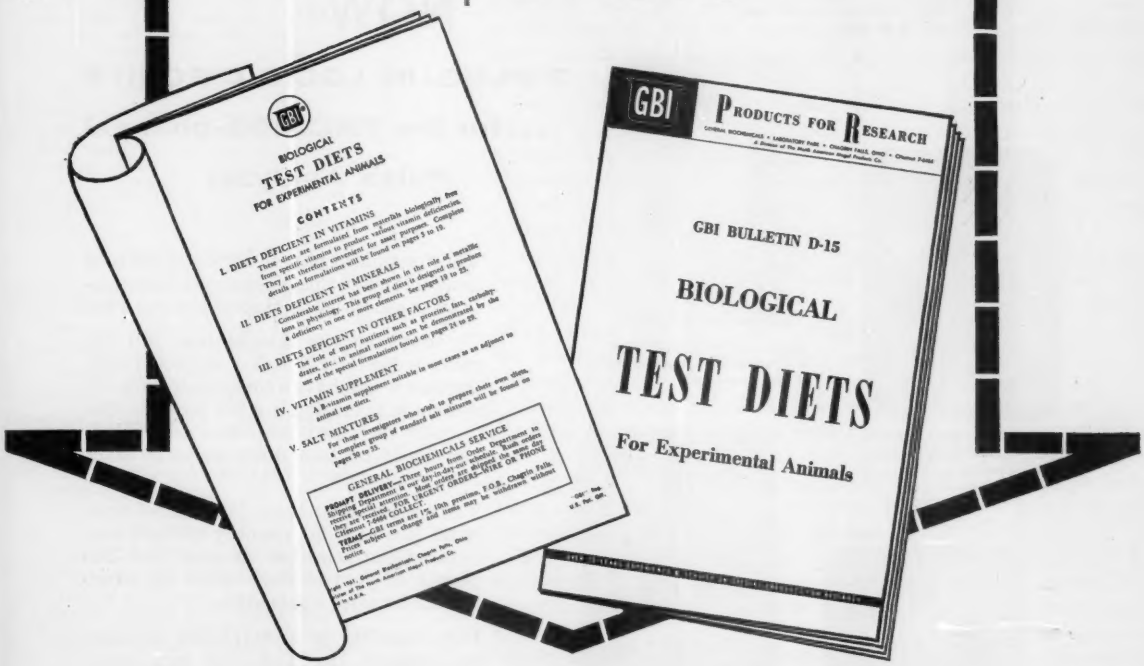

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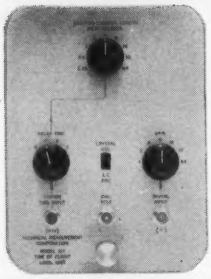

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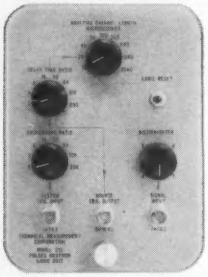

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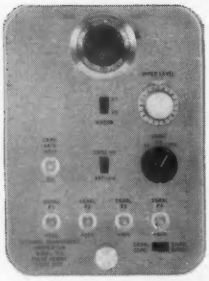

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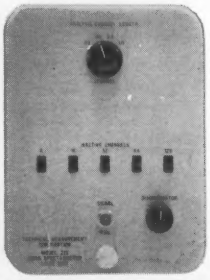

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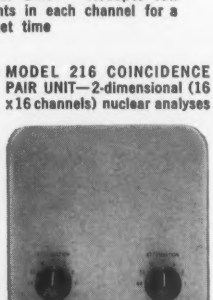

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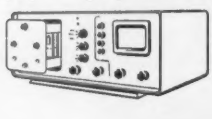

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A Matter of Perspective

If sending a man to the moon and bringing him back were entirely a scientific affair, then, as one suggestion has it, we ought to send a midget. Not only would the midget himself constitute a much smaller payload, but his requirements for food, oxygen, shielding from harmful radiation, and disposal of body wastes would be comparably reduced in scale.

Our present astronauts, it is true, do have the benefit of military training and discipline. But midgets who have had professional experience in circuses and carnivals will have absorbed the equally demanding traditions of show business. As for intelligence, Barnum's famous Tom Thumb was so bright that at the age of five he could be convincingly billed as an eleven-year-old.

This suggestion concerning our moon efforts would have been readily understood by the Eisenhower Administration. In those days American research in space was not regarded as part of a weight-lifting contest with the Russians. The problem was one of balancing the great costs of increased rocket power against the possibilities of miniaturization.

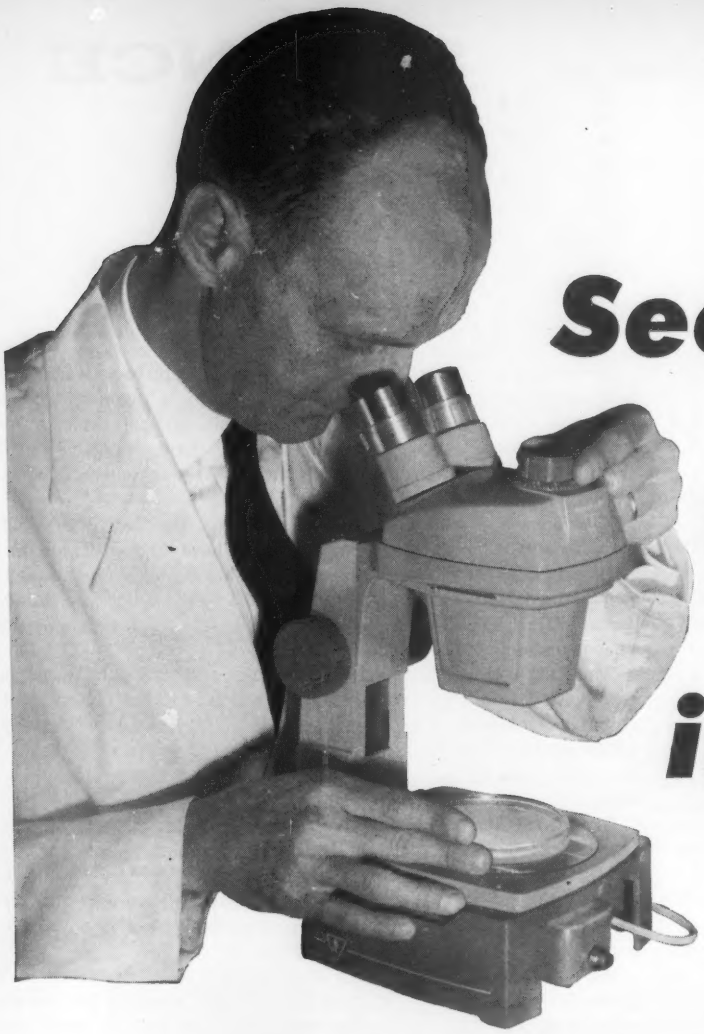
From such a viewpoint, we would have no cause to bow our heads in shame if we put one of the little troupers on the moon for a few days. He could do as much scientific work there as any normal-sized person. Besides, anyone concerned about national prestige who really looks into the matter can see that mere rocket power is not the best indicator of a nation's scientific prowess.

But times have changed. In the thinking that characterizes the Kennedy Administration, the trip is no longer regarded as entirely a scientific affair. There is a space race going on and we must win it in terms the judges can understand. The presence of an American midget on the moon, if followed by the appearance of a Russian of more ordinary dimensions, would still leave us running second.

In the matter of specifying costs, there is also a contrast between the two administrations. Under Eisenhower, no public statement was ever made concerning the cost of a lunar landing. Under Kennedy, we are given the bill. The cost (for a normal-sized person) over the next five years is put at $7 to $9 billion, with the total cost estimated at $20 to $40 billion.

Kennedy has kept his promise to discuss public issues honestly. Science stands to gain from the trip to the moon, possibly greatly. But the primary issue, as Kennedy says, is the effect of the undertaking on the struggle between the Soviet Union and the West. It may well be that midgets, like everyone else, should ask not what their country can do for them, but what they can do for their country. Their service, however, lies elsewhere.

Of course, even if we proceed on these terms we may still lose. The Russians, with their greater experience in matters concerning prestige and confidence, may turn around and send up the fattest fat-lady they can lay their hands on.—J.T.



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CURRENT PROBLEMS IN RESEARCH

Economic Potential of Communication Satellites

Satellites promise quantum jumps in overseas capacity at low cost if commercial traffic jumps too.

William Meckling

Man's first erratic sorties into space have afforded him an irresistible opportunity to indulge his fancy. The fantasy life he has conjured up encompasses both the technological possibilities and the potential benefits of space exploitation. In technological matters, "The difficult we can do now; the impossible will take a little longer," is accepted as a fact of life. Manned space platforms, moon bases, interplanetary travel, and the like are straightforward engineering problems—applications of known technology. On the social side the guiding principle is: "Benefits are directly proportional to romantic appeal." In brief, wishful thinking plays a leading role in the space drama.

A realistic appraisal, however, suggests that many a moon will set ere man establishes an encampment there and that the resources consumed in the effort will be prodigious. What is of greater importance, many more—perhaps an infinite number—will set before the investment is recovered. The same can be said for many of the other dramatic space endeavors. It will be a long time before they are realized, they will be costly, and it will be an even longer time before there is any significant payoff, if there ever is.

Amidst all this fantasy, however, there are one or two satellite applications that show definite promise of being near term. One of these is the use of satellites as communication relays. In purely physical terms, the advantage of communication satellites is the opportunity they offer for line-of-sight transmission over long distances. Because radio transmission in the atmosphere at the higher frequencies is limited to line of sight, microwave communication links at the surface of the earth must be fairly short—for example, 30 miles. For normal overland applications this requirement is not a serious disadvantage. Indeed, such links now provide at very low cost a substantial portion of our long-distance communication capacity. For transoceanic communications, however, microwave stations every 30 miles or so are impractical; and it is here that satellites come into the picture, for they enable us to have microwave links thousands of miles long.

Physical Characteristics of Communication Satellites

Basically, the idea of communication satellites is very simple. The satellite receives a signal radiated from earth and either reflects it or repeats it, for pickup by another ground station. Reflecting satellites are called passive; the Echo I balloon sponsored by the National Aeronautics and Space Administration is an example. Satellites that receive and retransmit signals are called active. Active satellites can be either immediate or delayed repeaters. Delayed repeaters receive at one time (and place) and relay the signal at a later time (and different place). The Army Signal Corps Courier satellite is an example. Immediate repeaters receive and transmit simultaneously and are called "real-time" repeaters. The Advent program of the Department of Defense is an example.

Satellites can also be classified according to orbital characteristics. The important distinction is between the so-called 24-hour equatorial orbit and all lower orbits. If a satellite is placed in orbit at an altitude of roughly 22,300 miles, its velocity will be such that it traverses a complete orbit every 24 hours. If the orbit also lies directly over the equator, the satellite will remain stationary over a specific point on earth. Satellites in lower orbits will be continuously moving with respect to points on the earth.

Satellites also differ in the extent to which their position and attitude may be controlled. At one extreme, the motion of the satellite and its orientation may be left entirely to the vicissitudes of orbital injection forces plus whatever natural forces are at work. At the other extreme, very sophisticated control equipment may be installed to control position and orientation precisely.

Obviously, a wide variety of communication-satellite systems is possible —passive, active, low-orbit, 24-hour-equatorial-orbit, real-time, delayed-time, attitude-controlled, position-controlled, and so on. Within each of these categories, of course, the number of variations in system design is virtually unlimited. There are variations in size, shape, and mount of antenna, in type of tubes, in orbital inclination, in number of satellites, in satellite weight and

The author is affiliated with the Rand Corporation, Santa Monica, Calif., in the economics department.

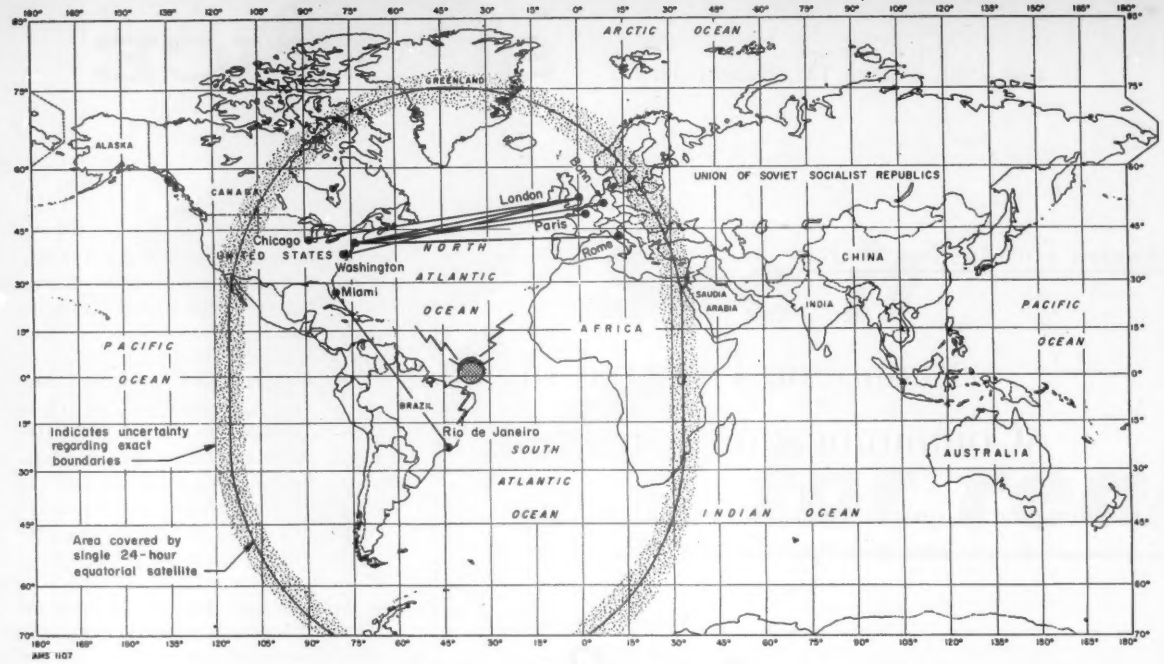

Fig. 1. Twenty-four-hour system links.

size, and so on. The various systems have different operational characteristics and pose different development problems.

Because of its great altitude, a single 24-hour satellite is simultaneously visible from widely separated points on the earth's surface (see Fig. 1), hence it could provide communication services over a large area. Three 24-hour equatorial satellites could provide continuous global coverage except for the polar regions. Such extensive coverage, however, also imposes penalties. For one thing, the energy required to place a given payload in orbit increases with altitude. For another thing, the 24-hour satellite will need position- and attitude-control systems to overcome natural forces tending to pull it out of orbit, as well as to offset any initial orbital placement errors. Position and attitude control adds to complexity and reduces reliability. The 24-hour equatorial system, because of its complexity and the requirement for large boosters, will probably take longer to develop than some of the other systems.

With lower-altitude systems the receiving and transmitting antennas at the ground terminals must sweep from horizon to horizon. They must focus on the satellites as they become visible over one horizon and follow them until they disappear over the other. As each satellite disappears, a new one, visible to both ground stations, takes its place. Because the area of simultaneous visibility is reduced at the lower altitudes, more satellites are required to provide continuous (or nearly continuous) service between any pair of ground terminals. In low-altitude systems the problems of development and reliability can be eased by dispensing with the attitude- and position-control systems, but this too increases the number of satellites required. Without position control, the satellites are randomly spaced, hence more satellites are required to insure that at least one is, at nearly all times, visible to each pair of ground stations.

All of the systems under serious consideration for commercial communications require very large receiving antennas and very sensitive receivers. The former must in effect be radio telescopes of the sort used by astronomers. The reason for this requirement is the low signal strength in the link from satellite to ground. With passive systems the reflected signal is quite weak at the distances involved, and even with active systems the power available in the satellite will be relatively small, at least in the near future. Because of this, it will be many years before satellites can be used for general broadcasting. Relay of television and radio programs from one terminal to another via satellite will be possible, but rebroadcast will be necessary for local audiences.

Uncertainties

The problem of evaluating the economic prospects for communication satellites is in principle the same as the problem of evaluating any other long-term production process. The development, production, and operation of a satellite system will require a stream of future expenditures and will provide a stream of receipts. Of course, a large number of alternative satellite systems might be developed. Moreover, each of them could be developed on a variety of schedules. Also, there are alternatives to communication satellites—for example, submarine cables—to be considered. If we knew the relevant expenditures and receipts profiles for each of the alternatives, we could determine the optimum program for providing future communication services.

Needless to say, in the real world we are going to have to settle for a lot less than this ideal. We are not going to forecast with much accuracy either the stream of costs or the stream of

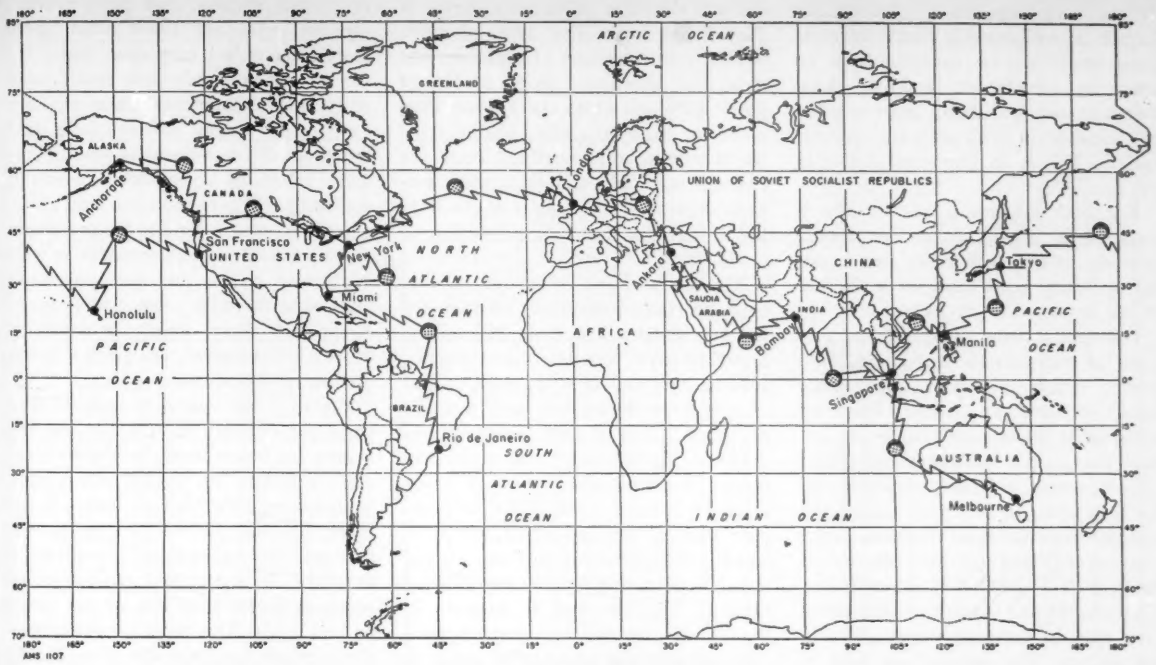

Fig. 2. Low-altitude-system links.

revenues for any system. The cost of various kinds of satellite systems depends on a host of technological questions that won't be answered even approximately until development has been carried much further than at present. What will be the useful lifetimes of various satellites? What will happen to launching costs? What will the probabilities of launch success be? How many usable channels can we really get out of given equipment? And so on.

Uncertainty about receipts is, if anything, greater than uncertainty about costs. Forecasting the demand for telephone and telegraph services 10 or 15 years in the future is difficult enough, let alone forecasting such things as demand for international television or data processing. In brief, until more evidence is available, conclusions must be regarded as tentative.

Costs of Communication Satellites

From the extensive array of candidates, two rather promising systems have been selected for analysis here. One is a low-altitude system; the other is a 24-hour equatorial system. Both are real-time active communication systems.

In the low-altitude system, there would be a maximum of 120 very lightweight active satellites in orbit at any given time. The satellites would be multiple-launched—that is, a number of them (say 40) would be placed in orbit with a single firing. They would not be position-controlled but would be dispersed around the earth in belts, with random spacing within each belt. It is estimated that one of these repeaters could provide either 600 *one*-way telephone voice channels or one television channel between a pair of stations. *Two*-way voice communication is achieved by using two different satellites visible simultaneously from two ground stations. Because of the large number of satellites in orbit, at least two satellites would be simultaneously visible from any pair of ground stations more than 99 percent of the time.

The ground network postulated consists of 13 stations encircling the world, as shown in Fig. 2. Since 600 pairs of two-way voice channels are provided between any pair of stations, the total capacity is 7800 station-to-station channels.

The 24-hour system considered here employs a single large satellite in a 24-hour equatorial orbit over the Atlantic. It is estimated that such a satellite could provide 4800 two-way voice channels. Part of the capacity (600 one-way channels) could be used for television, if desired. Service between seven pairs of stations is postulated—New York and London, New York and Bonn, New York and Paris, New York and Rome, Washington and London, Chicago and London, and Miami and Rio de Janeiro—as shown in Fig. 1. For global coverage three such satellites would be required, but the annual cost per voice channel (which is used here as a basis of comparison) would not be materially affected.

The two satellite systems present different problems from the point of view of service interruption. Because of the random spacing of satellites in the low-altitude system, two satellites would not always be visible from each pair of ground stations. This means that for any given link, interruptions of a few minutes' duration would be a daily occurrence. On the other hand, satellite failures would only gradually degrade the capability of the low-altitude system. As failures occurred, the interruptions would gradually grow more frequent and of longer duration.

With the 24-hour system, uninterrupted service would be provided around the clock except when a failure occurred. When that happened, however, it might be days or even weeks before service could be restored because

of the time required to successfully launch a replacement. Such interruptions would not be tolerable from an operating standpoint, and we have therefore assumed that a spare satellite will be kept in orbit with the 24-hour system. This is an important factor in the cost.

For both systems a 15-year life is assumed for ground equipment. Development costs, launch costs, and terminal operating costs throughout the life of the systems are all included.

Different systems entail different patterns of expenditure with time. Submarine cables require a large initial outlay relative to annual operating costs, while for satellite systems the annual charges for launching and operation represent a larger proportion of the total cost. In order to make valid comparisons between systems, costs must be adjusted to take account of different time profiles of expenditures. What this means is simply that interest costs must be correctly incorporated in the analysis. The interest rate used in this study is 8 percent.

Cost based on full utilization. The cost data are summarized in Figs. 3 and 4. The unit of measure is cost per (two-way) station-to-station voice channel per year.

In an attempt to take account of some of the uncertainties cost curves are shown for a variety of assumptions. The parameters which are varied are (i) mean time to failure (MTF), (ii) probability of launch success, and (iii) load factor. These are not the only important parameters, but only a limited number of variations can be presented here.

Figures 3 and 4 also show costs for submarine telephone cables and for the TD-2 microwave relay system. The TD-2 is an important type of overland microwave system used by the American Telephone and Telegraph Company for long-line communication channels. The microwave costs are system-wide averages based on the company's experience in the United States. As previously mentioned, microwave links are not practical for transoceanic communications; they are included here to indicate the competition satellites will face in *overland* transmission. Submarine telephone cables are the chief competitor of communication satellites in transoceanic communications. Such cables are themselves a new development. The first transatlantic telephone (not telegraph) cable was placed in service in 1956. The cable costs shown in Fig. 3 are for a new type of cable which the American Telephone and Telegraph Company plans to lay in 1963; they are substantially lower than costs for existing cables. Unlike costs for communication satellites, costs per channel for cable and microwave systems depend on the length of the link, hence they are shown as a function of circuit length.

Figure 3 shows estimated annual costs per (two-way) voice channel for the two systems, based on full utilization. The term "cost per voice channel," as used here and in subsequent discussion, is synonymous with "cost per unit of output," as that term is usually understood. For communication systems, output is measured in terms of voice channels *actually used*. Thus, to compute cost per unit of output, some assumption must be made about utilization. In computing the cost per channel (that is, cost per unit of output) in Fig. 3, the number of channels used (the output) was taken to be equal to the capacity of the system. Looked at from a slightly different viewpoint, the costs shown are the level annual prices that would have to be charged per voice channel in order to recover all costs, if all channels are sold every year. Thus, for the low-altitude system, when the mean time to failure of the satellites in orbit is 2 years and the probability of launch success is .75, a price of about $8500 per year per channel would just cover all costs, if all channels were sold at that price each year. For the 24-hour system, when the mean time to failure is 1 year and the probability of launch success is .75, a price of about $10,000 per year per channel would also cover all costs, if all channels were sold at that price each year. By comparison, for the new cables, the annual cost of a 3000-mile transoceanic link would be about $27,000 per voice channel. To put these figures in perspective—the telegraph companies pay $240,000 per year for one voice channel in the existing submarine cable. This bears out a point mentioned earlier: costs with the new cables will be substantially lower than costs with the present cables. Moreover, it appears that substantial reductions in toll charges should be possible—a fact that is of some interest in later discussion.

The ultimate potential of communication satellites is clear from these curves. Even with very pessimistic assumptions for mean times to failure, probabilities of launch success, and launch costs, satellite costs are lower than cable costs for any but very short links. On the other hand, only with much more optimistic assumptions than any we have made could we conclude that satellites will be competitive with microwave relays in conventional overland applications. There are some land areas, of course, where terrain factors might make a satellite system competitive with microwave relays, and some overland links—for example, from San Francisco to New York—might be available at very low marginal cost, given a global satellite communication system.

Figure 3 also seems to indicate that costs per channel for the low-altitude system are below those for the 24-hour satellite system for roughly comparable assumptions. (We assume shorter mean times to failure for the 24-hour system because of its greater complexity.) However, it would be rash indeed to conclude in favor of one or the other on this basis. The range of uncertainties is much too large for us to decide this question now.

Cost based on geometric increase in use. By present standards, satellites represent very-large-capacity international communication systems. In mid-1960 there were only 105 telephone voice channels between the United States and Europe and only 240 additional channels between the United States and other overseas points—345 in all. Even by domestic standards the capacity of communication satellites is impressive. At that same date, for example, there were some 850 channels between Los Angeles and New York and 2600 between New York and Chicago (*1*).

Thus a world-wide, low-altitude system with 7800 voice channels would provide about 20 times the number of transoceanic channels presently routed to the United States, and a single 24-hour transatlantic satellite with 4800 voice channels would provide about 45 times as many channels between the United States and Europe as are presently available. To appreciate the magnitude of these figures, one should remember that international telephone traffic to and from the United States in the last 10 years has increased by only a factor of 3.

For our purposes, the important point is that costs per unit of output, computed on the basis of full utilization, appear to be biased on the low side. One very convenient way to take

Annual cost per voice channel (thousands of dollars)

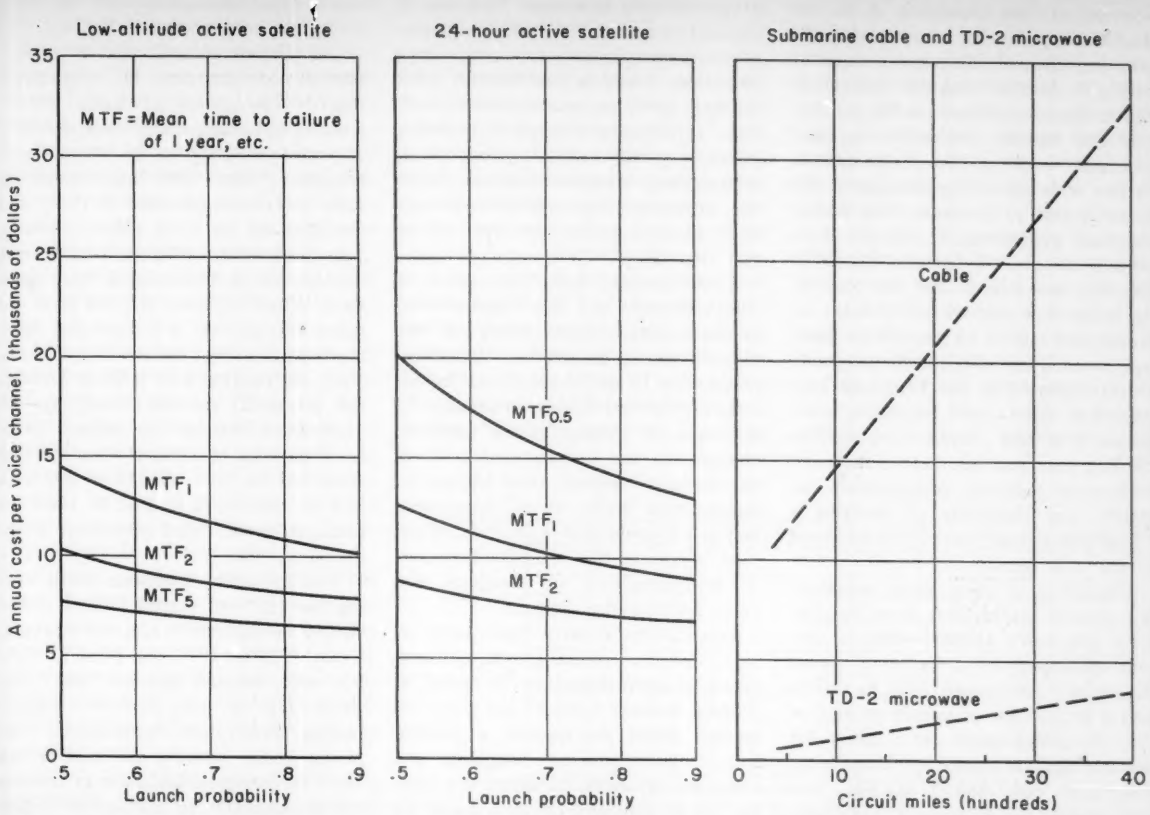

Fig. 3. Annual cost per voice channel on the basis of full utilization. MTF_1, average effective life of 1 year; MTF_2, of 2 years; and so on.

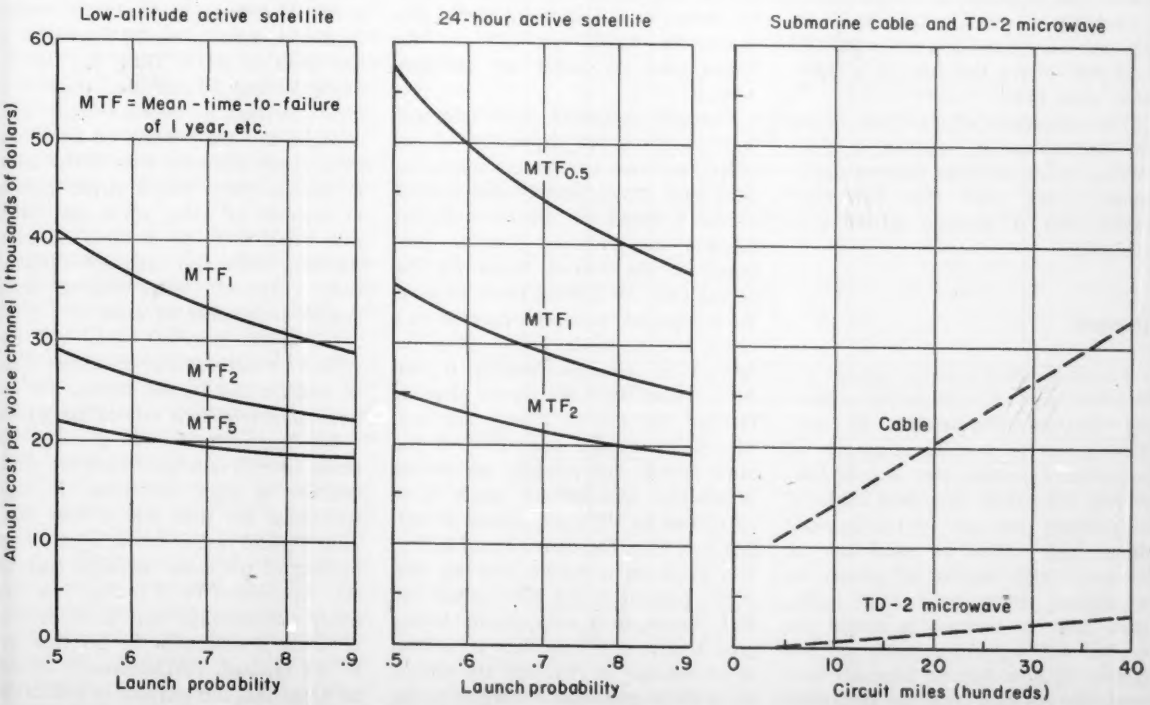

Fig. 4. Annual cost per voice channel on the basis of geometric increase in use.

account of this possibility is to use load-building factors in computing the cost per channel. This has been done in Fig. 4. Annual costs per channel, as shown there, are based on the assumption that satellite use builds up geometrically over the life of the system. In line with our earlier discussion, this is equivalent to assuming that output increases geometrically. For the low-altitude system it is assumed that 1000 channels are sold in the first year of operation and that thereafter sales increase at a rate of 15 percent per year, compounded annually, reaching capacity (7800 channels) in the 15th (and last) year. For the 24-hour system, it is assumed that 600 channels are sold in the first year and that there is the same 15-percent increase, compounded annually. The possibility of realizing a 15-percent annual increase is analyzed in the next section.

The effect of these more conservative growth assumptions is to multiply costs per voice channel—that is, per unit of output—by a factor of almost 3. For a 2-year mean time to failure and a probability of launch success of .75, the annual cost per channel for the low-altitude system is $24,000, as compared with $8500 in Fig. 3—only slightly less than the $27,000 cost per voice channel of a 3000-mile cable link. For the 24-hour system, with a mean time to failure of 1 year and a probability of launch success of .75, the cost per voice channel is about $27,500—slightly above the cost of a 3000-mile cable link.

The importance of high rates of use for communication satellites is clear. Without them, satellites promise significantly lower costs than submarine cables *only* if extreme reliability is achieved.

Demand

Communication satellite systems are in effect large-scale plants for producing communication services. As such, they appear to promise economies of large-scale operation; they promise lower unit costs than submarine cables if high output rates (or, what is the same thing, high utilization rates) can be achieved—high relative to current international telecommunication traffic. How large an increase in output the market will support depends on two factors: (i) how demand increases over time, and (ii) how sensitive utilization rates are to price reductions. Prices are particularly important. To judge by the current rate of $240,000 for a transatlantic voice channel, substantial price reductions should be possible with either the new cables or communication satellites. If consumers respond to lower prices by greatly increasing their use of international telecommunication facilities, communication satellites are very likely to be the more economical of the two alternatives.

Unfortunately, both the course of future demand and the responsiveness of the market to lower prices are very difficult factors to estimate. Economic projections 10 or 20 years into the future are always highly uncertain. In the case of communication satellites, changes in the international political environment, through their impact on international trade, travel, investment, and so on, could have a dramatic effect. New inventions that are substitutes for, or complementary to, satellites also could revolutionize demand.

Despite the uncertainties, however, an examination of the growth of international communications is useful in judging whether there is any hope for enough traffic to support a satellite system. Since overseas telephone and telegraph presently dominate the market, let us look at those first. Later we shall discuss very briefly some of the new uses that have been proposed. Although the use of satellites would not be limited to U.S.-centered traffic, this component would probably be the largest, and we center our attention upon it.

Overseas telephone and telegraph services. Since the depression of the 1930's, overseas telephone communications have grown steadily and rapidly. Figure 5 shows the annual totals for inbound and outbound overseas messages for the United States for the period 1930–59. During those 30 years the number of messages increased by a factor of 100. In the last 10 years the volume of messages roughly tripled. As is evident from the regular slope of the line, the rate of growth has been remarkably constant since 1946. If that same growth rate persists, the volume of overseas messages will reach 10 to 12 million by 1970 and around 40 million by 1980. The dotted line *B* in Fig. 5 is based on a growth rate like that of the postwar period. The dashed line *A* is based on a substantially higher growth rate, which predicts 20 million or so messages in 1970 and 100 million by 1980. *A* corresponds roughly to the assumption of a 15-percent annual increase—the assumption used in computing the costs shown in Fig. 4.

The history of toll rates is an important consideration in interpreting Fig. 5. The period of stable growth from 1946 to the present coincides with a period of stability in international telephone rates. The last change in rates to London occurred in 1945, and though rates to some other countries were lowered in 1946, there have been no changes in international rates since then. When increases in price level are taken into account, it is clear that there has been a modest decline in real rates. Thus the real price of calls to London fell about 20 percent during the 14 years from 1946 to the present. Prior to 1946, rates to London were reduced from $45 in 1929, to $30 in 1934, to $21 in 1936, and to $12 in 1945. In addition, special night rates were introduced in 1936.

The growth of telephone traffic during the 1929–45 period follows rather closely the pattern of real rate changes. From 1929 to 1934 real rates to London rose, because nominal rates were fixed while the price level was falling. During those years international telephone traffic actually declined. From 1934 on, however, real rates to London fell dramatically, so that by 1946 they were about 20 percent of what they had been in 1929. These were also the years during which telephone traffic experienced its fastest growth. In 1946 message volume was about 20 times what it had been in 1935. Thus, for the 30 years covered by our data there is a high correlation between real rate changes and the volume of telephone calls. When real rates were fairly stable (1946–59), there was a stable growth in number of calls; when real rates rose (1929–34), the number of calls declined; when real rates fell substantially (1935–45), there was a rapid growth in number of calls.

In brief, historical data on the growth of international telephone traffic indicate that (i) the demand for international telephone service has grown rather impressively over the last 30 years, and (ii) utilization rates are quite sensitive to price reductions. If both statements are true, and if they continue to hold in the future, a growth in number of telephone messages such as that represented by *A* in Fig. 5 is certainly a possibility—that is, of course, if potential reductions in toll rate are in fact realized. Furthermore, it should be noted that the increase in utilization will exceed the increase in message

volume. Estimates of message volume by itself ignore one dimension of utilization—namely, the duration of calls. Not only will the number of calls increase if toll rates are reduced, but the average length of calls will increase.

In terms of both revenue generated and message volume, international telegraph traffic to and from the United States exceeds telephone traffic. For a number of reasons, however, the telegraph business is likely to play a minor role in the communication-satellite picture.

First and most important, much less communications capacity is required for telegraph transmission than for telephone. One telephone voice channel is roughly equivalent in capacity to 20 telegraph channels. Thus, telegraph is a modest consumer of the one thing a satellite system provides—large quantities of long-line capacity.

Second, with telegraph there is less opportunity for increasing utilization by reducing toll rates, because long-line transmission costs are a relatively small proportion of the total cost of a message; reductions in transmission costs will not appreciably affect the cost per message.

Third, telegraph also looks less promising from the point of view of traffic growth. The volume of telegraph messages to and from the United States is only about 25 percent greater today than it was 30 years ago, though all of that growth has occurred since the end of World War II. Certain special forms of telegraph service—private leased lines and Telex (a means of conducting conversations by telegraph)—have been growing rapidly, but these constitute a relatively small part of the total.

Finally, in many parts of the world where telegraph is important, communication-satellite channels will not be available for telegraph applications. Because of their high cost, the number of satellite ground terminals will be limited (most countries simply won't have terminals), and, where there are no terminals, telegraph traffic cannot be channeled through the satellite system.

Proposed supplemental uses. No consideration of the demand for communication satellites would be complete without a discussion of the varied assortment of supplemental uses that have been proposed—for international commercial television, data transmission, facsimile transmission, closed-circuit television, and so on. In the fields of international telephone and telegraph, we at least have a history to rely on, but there is virtually no experience to help in evaluating these proposals. What can be said is, therefore, even more tentative and less quantitative than predictions in the other two fields.

Commercial television is the use that has received most attention. The operational difficulties in international television are well known by now. Time and language differences limit the market. For much of the world, simultaneous broadcast of the daily fare would be out of the question, and even briefly delayed broadcasts, such as those used for different time zones in the United States, would not be feasible. Special events like the Olympics could be broadcast simultaneously to some areas. But even here, delayed broadcast by tape would probably be more satisfactory. For delayed broadcast the question is: What advantage would satellites offer over delivery by fast aircraft—that is, how much more would television sponsors be willing to pay for satellite transmission? Such transmission is going to be costly. Even with favorable assumptions, it appears that the annual cost of 600-voice channels in a communication satellite will be substantially larger than the cost of domestic microwave relays.

One important possibility for encouraging new uses, such as delayed television facsimile transmission, and

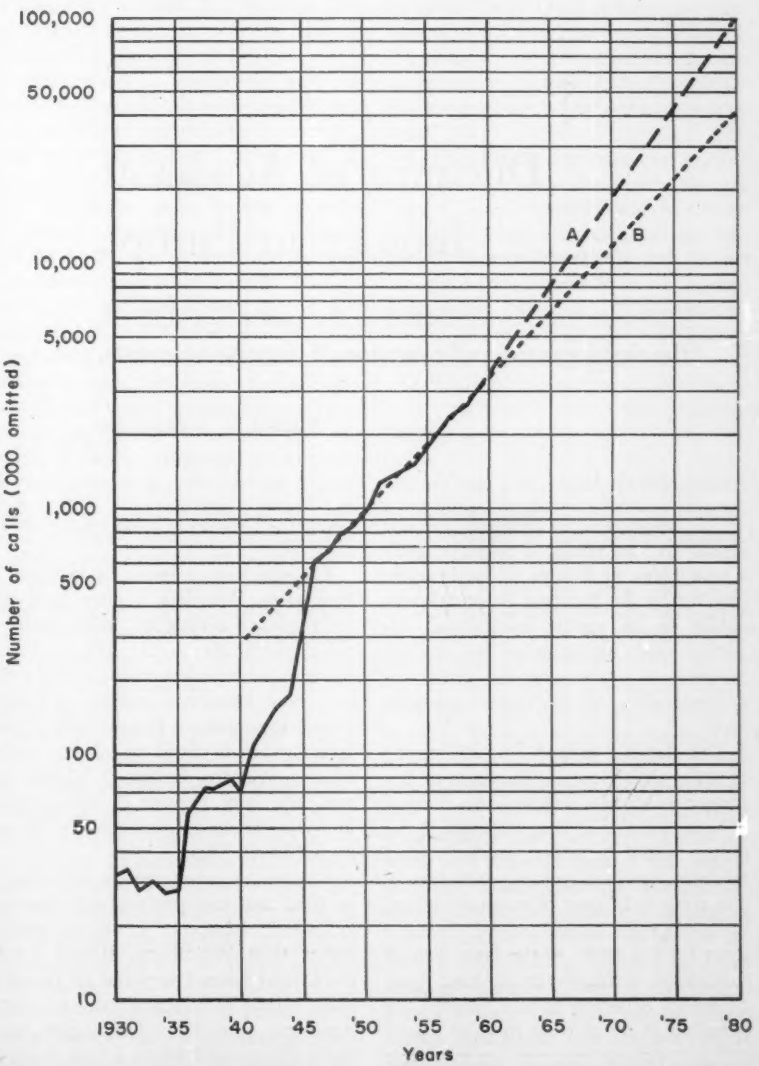

Fig. 5. Overseas telephone calls per year to and from the United States for the period 1930–59 and projections based on two growth rates (see text).

data transmission, is to inaugurate a policy of varying prices with the time of day. During "off-peak" periods, communication-satellite systems will have large "excess capacity." Once the system is in operation, the cost of using the excess capacity will be slight. Thus, it may be possible—say, during the night—to use the satellites for transmitting facsimile mail and taped television or data at very low prices. Unless some such arrangement is made, the potential demand for such uses is likely to be small.

If a low-cost method is found for bringing closed-circuit television into homes and offices, this could generate an important demand for international communication capacity, probably a much more important demand than that of commercial television. Though much more expensive than telephone, it would provide a close substitute for travel for many business and official purposes.

Thus new forms of international communications—probably some kinds that are not recognized today—will add something to the demand for capacity. Whether they will dramatically increase total demand or will have only a modest effect is hard to say.

Communication satellites promise substantial reductions in cost, as compared to the new submarine cables, if high rates of utilization can be achieved. The real question, therefore, is demand: Just how soon will demand be sufficient to support such systems? If we judge on the basis of past growth of international telephone traffic, plus possible channeling of telegraph traffic and potential new uses, that day may not be too far in the future. That does not mean it will be next year, however, or even 1965, but some time thereafter (*2*).

References and Notes

1. F. R. Kappel, "Communications in the space age," lecture delivered at the University of California, Los Angeles, 29 June 1960.
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Dilemma of Academic Biology in Europe

University customs steeped in the past make difficult the development and retention of creative scientists.

William V. Consolazio

The observations reported in this article are the result of approximately a year's stay in Europe, recently made possible by the National Science Foundation in its newly inaugurated advanced study program set up for the purpose of augmenting the skills and understanding of its career scientific staff.

The deeper I probed into this survey of the organization and the needs of European academic biology, the more I became convinced that whatever I reported would be a first approximation to a most complex and singularly important subject. In spite of extensive effort, the time I spent abroad proved much too short for the needs of the task. Several specialized biologists could have been profitably occupied at this chore, each spending as much time in each of the countries I visited as I did on my entire trip.

I would like to begin by defining a few terms. Although I have used the term *science* throughout this article, I refer specifically to the biological sciences in the broadest sense. I have written about European science. Actually, I refer to science in Europe west of the Iron Curtain. I visited all but two countries in Western Europe. I include Israel, not only because I visited there but also because, intellectually, Israel is part of the West.

I visited as many centers of research as time and my professional interests permitted; however, I visited people rather than institutions. Before I left the United States I prepared an itinerary based on the recommendations of many American experimental biologists who know Europe and whom I hold in great respect.

It is a truism that science is international; it knows no national boundaries. It is also axiomatic that whether or not there is science in a country, or whether there is a good or a bad science, depends on the number, quality, and training of the nation's scientists, the availability of research funds, the extent of the nation's educational facilities, and the state of its research equipment and laboratories. But more than this, the nature of a nation's science depends on the intellectual and scientific traditions and attitudes of that country—I would say on the intellectual and scientific receptivity of a nation. One thing is clear: no nation has a monopoly on intelligence and on scientific potential. Bright young men and women are just as common in the more unfortunate and poverty-stricken countries as they are in the wealthier and intellectually more highly developed nations. The only difference between the haves and the have-nots with respect to scientific potential lies in the fact that in the more fortunate countries young people are better trained and utilized, and that the potential is thus more fully exploited. Despite our self-castigation, we in the United States don't waste nearly as many young people as are wasted in Central and Southern Europe.

It is well known that, of the countries I visited, science is most highly developed in the British Isles, in Israel, and in Sweden. There are pockets of scientific activity of very high quality in most of the other countries of Western Europe, but on the whole one can say that as one proceeds south, both the quality and the quantity of scientific activity decline. But let me add

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quickly that the best southern European scientists are as good as the best Swedish and British; they are fewer, and they work under considerably more stressful and less rewarding conditions. Southern European scientists are considerably more isolated; they have heavier teaching loads; their facilities are more primitive; their pay scales are very low; and the student body is unselected and is rarely trained to follow a career in science.

The most productive European scientists I met all had a common training. In general, they had all been trained in the same scientific centers, both in Europe and in the United States. All came from a common mold and had become siblings through education. It also is a fact that all productive scientists in Europe read and speak English, most of them fluently. English and American journals and monographs predominate in their libraries—an indication of the dependence of European science on English and U.S. publications and of the universality of English in the scientific world.

Of great interest in this connection is the significant number of productive European scientists who have at some time in their careers received support from the Rockefeller Foundation, either in the form of a fellowship or indirectly through grant funds. This record of the Rockefeller Foundation is one of which Americans should be exceedingly proud. It also demonstrates the value of the grant-in-aid system of supporting science. Rockefeller Foundation funds have always been in very short supply. They have always been employed as catalysts, and the high points of European science attest to the past effectiveness of this system.

World Wars I and II played a significant role in the decline of science in Central and Southern Europe. The disruptive effects of the two wars, of course compounded by Fascism and Nazism, drove some of the best experimental biologists out of Germany and Italy. A few of these experimental biologists went to the British Isles and Scandinavia; the majority came to the United States. Europe still has to recover from this catastrophe. But it seems to me that Europe has lost its dominance in science for reasons other than the wars. Independent developments in the new world; nationalistic, linguistic, and cultural barriers; and economics—all have taken their toll. It is to some of these issues that I address myself.

Why Biologists Leave Europe

Scientifically, Europe is being outstripped at a relatively fast rate by the United States. Although Great Britain, Sweden, and Israel are increasing their research activities, by comparison with the increase in the United States, the increment is small. This results in the emigration of far too many able young people from Europe to the United States. Very few Americans seek permanent scientific employment in foreign countries, with the exception of those who seek it in Israel, so that the movement of scientists is practically all in the direction of the New World and, primarily, of the United States. Emigration begins when young people seek further training abroad. Exposure to the great scientific centers of the United States and a taste of our high standard of living exert an irresistible pull on them. By contrast, a return home often means a lack of opportunity to grow intellectually and difficulty in supporting oneself and one's family. It is logical, then, for these young people to seek permanent residence in the United States.

Europe has neither enough trained people nor the facilities to exploit the many scientific breakthroughs that still occur there. This discouraging state of affairs is very frustrating to an individual who makes a fundamental discovery and then has to stand by and watch it being exploited in a prosperous country like the United States. Those who have had this experience once or twice soon break out and leave.

With respect to the criticism leveled at the New World for proselytizing these young people, my answer is that society is the beneficiary when there are nations that, from whatever motivation, give assistance to deserving young people.

However, we in the United States should make every effort to encourage the young people who come here for training to return home when they have completed their training. I believe we should do it by persuasion and by helping to modify conditions at home and not by legislation. Nevertheless, it is good for Europe to find the United States competing for its young people. This competition forces political and educational authorities to focus attention on the problem of emigration. When attention is drawn to the problem, reforms are inevitable. We ourselves can help these returning young people by giving them financial aid until they become established in their home environment. It does no good to train them in the techniques of modern experimental biology and then banish them to a desert to count sand particles.

The late Enzo Boeri, who had to live with the problem, described it more dramatically than I can. "History teaches us that when freedom of thinking, speaking, and writing was acquired, a second step appeared necessary: freedom from want. What is the use of my freedom of thinking, speaking, and writing—claimed the poor—if I starve? What is the use of my freedom of thinking, speaking, and writing if I am a teacher of experimental science and I have no money to plan and make experiments? We must extend the old concept of academic freedom and take it also to mean freedom to act, freedom to perform one's academic function without being compelled by poverty to inactivity" (*1*).

Unfortunately, any system of foreign aid that absolves the home country from financial responsibility for establishing its young people encourages further irresponsibility. It may well be that foreign aid should be tied to a matching-fund system irrespective of whether the aid or the matching-fund comes from private or public sources.

Science in Israel

The size, population, and economic status of a nation bear little relationship to the quality of that nation's science. In my opinion science in Israel—a relatively new state not much bigger than New Jersey, with a population of some 2 million and with phenomenal social and economic problems still to be solved—is of higher quality and better developed than that in any country in Western Europe, with the possible exception of Great Britain and Sweden. The Israelis have harnessed their energies collectively to raise the intellectual standards of their country. Their political and scientific leaders are responsive and responsible. This is a country where intellectual ability is held in very high regard, and where the people are willing to impose on themselves a tax structure which is practically confiscatory to raise the standards of living, higher education, and science. These people make the most of every foreign opportunity available to them. They spare no effort or expense to send

their best young people abroad for training, and then they make it advantageous for them to return. As I said earlier, Israel is the only country that has been able to attract U.S. scientists as permanent residents. Part of this, of course, is due to the fact that some American Jews have identified themselves with Zionism, but a good deal is also due to good working conditions in Israel. Scientific tradition in Israel is but one generation old. But Israel has imported and adopted the best of the Western scientific tradition—scientific honesty, respect for merit, self-criticism, and rewards based on competence.

Traditions and Nationalism

Italy and Southern Europe, on the other hand, where the scientific tradition is old, have retained many of the symbols but few of the ideals of science. I am convinced that the "institute"—the administrative unit for academic science and education—is partly responsible. It is headed by a director who is also, except in rare instances, the only full professor. An institute may contain any number of staff members, varying from one or two (usually the rule) to numbers that approach those of our more highly developed university departments, but staff members are usually junior and subservient. The few able and productive directors make a substantial effort to develop an attractive environment for a creative staff, but attractive environments are the exception rather than the rule. The director of an institute holds power of livelihood and promotion over his assistant. As someone put it, the institute directorship, at least in Southern Europe, constitutes the last stronghold of feudalism. The director is a potent force because in Europe the university is usually a federation of colleges or institutes, or both. He holds membership in the academic senate, which is in most cases the administrative and executive arm of the university. Rectors and deans are rarely selected for executive ability; positions are usually honorary—a relic of the pomp and pageantry of the past. It is this academic senate that stands in the way of many of the reforms that are so badly needed in the European university.

Junior faculty members seeking permanent university appointments tend to cultivate the good will of the institute director and of members of the academic senate rather than to concentrate on scholarly accomplishments. It is the tragedy of many European universities that their young people must cater to the powers that be if they are to receive rewards. In a system where youth is subordinate to age and the elder scholar happens to be a brilliant individual, the young man may suffer but society and science nevertheless gain. If, on the other hand, the same youth is exploited by mediocrity, everyone loses. Unfortunately, in any society mediocrity is more apt to be prevalent than genius.

In many of the old and great medical schools of Europe there are still no institutes or departments of biochemistry and microbiology. Every European university has an institute of zoology and one of botany, but none of biology. Under the present system, if one wanted to introduce genetics, embryology, microbiology, or biochemistry into a university curriculum, it would be necessary either to create new "chairs" (new institutes) or to see to it that an individual who received his earlier training in an older discipline seeks further training in the newer one. Institute directors will be likely to resist appointing, for example, a biochemist or microbiologist to an existing "chair," such as human physiology or comparative anatomy, unless the candidate began his career as a human physiologist or an anatomist. Efforts to introduce the newer disciplines through this system have not been very successful.

Even more resistance greets proposals to create new institutes. Creation of new institutes would encroach upon the prerogatives of existing institutes and directors and threaten the present power structure. The professor's salary still depends to a large degree on the number of students registered in his course. In many European medical schools the funds resulting from clinical analyses (blood and urine analyses) revert to an older department within the medical school. It would be natural for the professor of an older department to resist the creation of new institutes with which such funds might have to be shared. There is also severe competition in the graduate faculty for the few graduate students. No one wishes to diminish the number of students in his own course by sharing them with a new colleague.

Does the size of a nation bear any relationship to the breadth and depth of its science? The fact that a nation is small does affect the type and the variety of its science, the example of Israel notwithstanding. It is economically impossible for a small nation to support research of the breadth and depth of that going on in the United States today. One finds very highly advanced physical and chemical biology in Sweden, for example, but one doesn't find the same depth in the other branches of biology. One finds some of the world's best intermediary metabolism and protein chemistry in Great Britain, but not an equally potent genetics. It is economics, to a large extent, that stands in the way of developing breadth. But economics is by no means the only or even the primary factor. Nationalism and the limitations imposed by the manpower pool play an even more significant role. The manpower limitation, however, could be overcome were it not for nationalistic barriers. The European environment is not conducive to the importation of scholars from foreign countries. In the United States, academic and scientific personnel at least cross state boundaries. Such exchange, of course, is the source of our national strength economically and of our scientific strength, too. But in Europe scientists do not often settle in countries other than their own. The many national and intellectual barriers get higher and more difficult to surmount, with the result that European science becomes more isolated and provincial.

European universities are old. Some go back to the 10th and 11th centuries. They began as institutions for the training of the clergy and were used later for training in professions associated with the humanities—law, linguistics, literature, and so on. It seemed to me that too many European universities are much as they were when they were established; they have changed very little. They are still dominated by classicists. Descriptive biologists still speak for biology, and they cling to what they have. Just as long as these groups are dominant, they are a power to be reckoned with. Classicists and descriptive biologists tend to delay modernization of the university to accommodate modern science and seem fixed in their belief that one professor suffices for a department. In many respects the professors in old-line disciplines fulfill their responsibility to the university by their teaching. They need nothing for their research but their collections, the library, paper, and pencils. Consequently they seem to have little appreciation of the nature and needs of modern experimental science. Modern laboratories and the establishment of a departmental

structure are matters foreign to their understanding.

The academic atmosphere in Europe is somewhat more relaxed than its counterpart in the United States. In spite of this, in the past Europe has produced much of the great science of the world. There is no paradox here—all agree that great thoughts are nourished in a leisurely atmosphere. The combination in the past of a relaxed atmosphere at the summit and an ever-replenished pool of youth dedicated to furthering the aspirations of the leader proved most profitable. Unfortunately for this system, young people the world over are in rebellion against exploitation and especially against traditional ways that perpetuate the status quo. But although there is a stirring and an unhappiness, little change will occur till the youth are encouraged to develop independently, till those who have emigrated are encouraged to return home, and till scientists and teachers are encouraged to cross national and disciplinary barriers.

For comparison, one can point to the fact that no small part of Israel's scientific success can be traced to its spirit of internationalism. Its scientists and its science are truly international. Our strength here in America stems from a highly competitive system and from the same international outlook. As long as universities in the United States continue the practice of competing vigorously with each other and with international universities for the best minds, just so long will our science remain strong.

Economics of Academic Biology

The creative life of the university in Europe is still best suited to the upper-upper classes, those of noble birth and wealth. Not too long ago those seeking careers in the university were expected to be individuals of private means. The university was not an employer of educators and scientists, as it has come to be in the United States. Many universities still fail to realize that teaching and research are professions associated with a livelihood. Consequently, academicians without private means of support must look to extracurricular sources for their living, for the university is often incapable of meeting its financial obligations to its employees. There is no longer a place for the dilettante in science; science has become much too complicated, too expensive and competitive, and government, the major source of support of European universities, has not yet assumed its full responsibility.

In 1953 Italy spent $23 million on its universities and high schools (*1*). This amount was to meet the educational needs of a population of approximately 40 million people. These figures include all expenditures—for buildings, salaries, libraries, research, and so on. If I can trust my memory, figures for the United States for higher education alone and for the comparable period were of the order of $3.5 billion. The U.S. expenditure was for a population approximately 4 times the population of Italy but, as I said, for higher education only. What can one do but keep salaries low if the funds available for education are so pitifully small? And how can we expect more than one professor per institute? Under these circumstances, the talented youth without hope of tenure or the security of a sponsor would be foolish to elect academic science as a career, for he could never give his full energy to research and teaching. To expect to do so would be unrealistic.

Except in rare instances, all support for higher education in Europe comes from taxes. Consequently, salaries are generally standard and comparable throughout universities and throughout the civil service structure. Therefore, there is little incentive to move from university to university and from civil service to university, and vice versa. There is still too little competition from industry, since there is little intellectual and social reward associated with an industrial career. The academician is held in great respect in Europe, but prestige in itself is insufficient to make the academic career attractive. I do not want to create the impression that salary is the only motivating factor in a scientist's life, but where the standard of living is low, money takes on great importance. Under low-income conditions creative people who are wholly dependent on salary will and do seek other professions or leave for greener pastures.

A Need for Change

Governments in Europe, it seems to me, have leaned over backward in their effort not to interfere and have deferred too much to university committees, sometimes piling one committee on top of another and thus further weakening university administration. The academic world in Europe is thus without "executive" officers who could become responsible for modernizing and upgrading the university.

I am convinced that the individualistic, "efficiently" administered American university, both public and private, with all its disadvantages, is to be preferred above any of the types of institutions of higher learning I have seen in Europe. If nothing else, American universities at least offer variety and competition.

Ways must be found to upgrade science and the teaching of science in Europe. Modification of the existing university structure offers hope. But this avenue of approach from within is slow and painful. The most logical and most effective device lies in the development of international institutes of science and technology. Those Europeans who think about such problems strongly advocate such a development. Institutes of this kind are the Technion in Haifa and the Institute of Science and Technology at Imperial College, London. A study group of the North Atlantic Treaty Organization has recently advocated similar developments (*2*). In contrast to the over-all institute, covering science and technology, establishment of specialized institutes has been suggested, such as institutes of cell biology. The advantages and disadvantages of these approaches have been discussed by many, and I do not consider them here. Suffice it to say, I feel quite strongly that international institutes, to serve as model sysems, offer the only real hope of change, and that the more specialized institutes hold the greater promise.

These model institutes should be under international management and, preferably, funded from private sources. At the beginning they ought to be staffed by nonnationals. Only an international institution can be free from local economic pressures and from efforts of competing institutions to attract teachers and scientists of high caliber. Foreign professional staff is as essential as foreign management if tradition and local political considerations are not to have the upper hand. At present, local customs are steeped in the past, pay scales are too low to make academic science an attractive area of employment, and local talent tends to be too provincial. In my judgment only the establishment of an international institute can shake European scientific education to its roots, and only an international institute can show Europe the modern way.

One frequently hears it stated that the way to uplift European biology is to put more money into the system. Heaven forbid! It does no good to develop programs of general support to science in "depressed areas," including some areas of Europe, without first introducing and fixing within the populace those scientific ideals that reward honesty in science, that recognize the value of self-evaluation and self-criticism, and that place scientific accomplishment above all other gains. These ideals are the core of the successful scientific environment. Scientifically "depressed areas" need assistance of a bread-and-butter nature. They don't need caviar. They need teachers, textbooks, journals, audio-visual aids, and the like. They need scientific instruction and examples of the "scientific method" at the most primary level. Giving research funds of a general support nature to existing institutions would only continue the present intellectual stagnation and perhaps even deepen it.

International institutes of cell biology can bring some of the ideals of science to these "depressed areas" and can serve as models, setting standards of research and education now lacking in all but a few nations of Europe. Through them young people would be exposed to the highest ideals of science, trained in the most recent techniques, and thus enabled to take over the training of subsequent generations. To my way of thinking, one model institute strategically placed would be of far greater value than all the foreign grant-in-aid, training, and fellowship assistance now made available to each of the countries I visited.

U.S. Aid for Research and Training

Of the many problems facing European science, a few can be solved with money. The amount isn't very large by U.S. standards, even though it is for too large to be attainable from present European sources. It is not our responsibility to underwrite European academic science, but it certainly is in our national interest to render some financial aid. We need a strong European science not only because Europe still is a producer of many original ideas but because we need competition to keep our own science healthy and dynamic. We also need training centers for seasoning our young people—training centers, different from our own, where, for example, the mind gets greater play than the machine. The Europeans have had to make do. Lack of funds to some extent has been a blessing in disguise—it has forced Europe to stress conceptualization rather than experimentation.

The kind of aid European science needs from us is of two types—support for its best young people and support for a few of its more creative scientists. Some of this we are already giving. Support for the young people should be in the form of postdoctoral fellowships for training in the United States and of support for a few years of subsequent research to help establish them at home. A fellowship program without some support at home is of little value. The Rockefeller Foundation was very successful with this type of program a generation ago, and it should not be too difficult for us to emulate its success. The other type of program can be in the nature of grants to help the more creative people establish and maintain training programs. There are very few postdoctoral students in Europe—first, because few training centers exist, but even more because there are few sources of funds to support these young people during an additional few years of training.

Of course we can make available textbooks, journals, audio-visual aids, and other teaching essentials. These items are desperately needed, but federal and private U.S. overseas agencies exist that can handle these needs. We can, however, make available our surplus, unused scientific equipment and periodicals now gathering dust and deteriorating in a large number of laboratory storerooms throughout the nation. A small investment to recondition and redistribute this equipment will gain us many friends and at the same time help familiarize many young people with laboratory techniques with which they are now unfamiliar. A simple expansion of existing book exchanges can accomplish the rest. Europeans are too desperately in need of scientific equipment and periodicals of any kind to let pride stand in the way of acceptance.

Whatever financial support is made available to Europe from U.S. sources should be direct—from the primary agency to the scientist. It should come from many of the same agencies, both private and public, that are now functioning in the United States, and certainly not from any specially created agency of the federal government. For whatever reason, we now tolerate a multiplicity of agencies for the support of science in the United States; that same reasoning obtains for Europe. But I repeat—aid should come directly and not through any middle-man international organization.

Europe Can Help Itself

Europe can begin to help itself too, for the manpower it is wasting constitutes a valuable and irreplaceable natural resource. European economic recovery has been dramatic. European citizens of wealth, industrial corporations, and governments have a responsibility to finance the education of their youth and the research of their scholars. Public organizations do now exist, but they are much too inadequately financed and managed for the task at hand. Tax incentives will, to some degree, further the development and growth of philanthropic organizations. It will take Europe a long time, even with the best of intentions, to approach anything like the present American response to the needs of its science.

Before I close I should like to leave one more thought. Anyone who has traveled about Europe or in other parts of the world in the last few years is aware that the world is in a deep ferment, politically, socially, and economically. Fundamentally, as in all such awakenings, it is the young people who are stirring. They are aware of the strangle hold of tradition and the dead hand of the past, and they are losing patience with delays and, yes, even obstruction. We, one of the youngest nations of the world, one born of rebellion, the most powerful economically and intellectually, can easily afford to share a bit of our good fortune and wealth with them. Any assistance we can give them will eventually be of benefit to us as well as to them. Such aid may spell the difference between survival of life on this planet as we now know it or total destruction. Will it be possible for responsible people and nations to move before the help provided is, once again, too little and too late?

References

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The Humoral Regulation of Breathing

A concept based on the physicochemical composition of mixed venous and arterial blood is presented.

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Few problems in physiology have been more resistant to explanation than the mechanisms which regulate breathing during exercise. In this article the humoral aspects of these mechanisms are presented in terms of a new concept that accounts for the hyperpnea of exercise and also the ventilatory responses to acid-base imbalance (*1*). Here we consider humoral regulation only, with full realization that breathing can be controlled voluntarily; that it is altered by extraneous factors such as posture, emotion, pain, and other stimuli; that there are physiologically significant respiratory reflexes mediated by baroreceptors in the circulatory system; and, that, for a given ventilatory requirement, the regulation of respiratory frequency, in relation to tidal volume, is governed by neural mechanisms in the "biologic oscillator" which comprises the "respiratory center" (*2*).

We have approached the regulation of breathing during exercise in an unconventional fashion. Whereas other investigators have evaluated the ventilatory response of the resting organism to various stimuli, such as inhalation of carbon dioxide and injection of acid, and have applied the results of these observations in attempts to account for the hyperpnea of exercise, we have done the opposite. A quantitative description of the physiological and biochemical responses to exercise has been developed and applied to several situations characterized by hyperpnea. This description of several concomitants of exercise not only predicts the ventilatory response to exercise but also predicts, with acceptable accuracy, ventilatory responses in the other situations characterized by hyperpnea, provided one postulates a peripheral chemoreceptor mechanism which reacts to the composition of mixed venous blood.

In its present state of development the concept is crude, mathematically. It demonstrates, nevertheless, that the ventilatory response to exercise represents a general pattern of physiologic reaction to the composition of mixed venous and arterial blood.

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Currently Accepted Theories of Respiratory Regulation

Since there are several recent reviews (*3–8*) of the various theories that have been proposed to explain the regulation of breathing, these will not be re-examined. The principal features of most of these theories are shown diagrammatically in Fig. 1*A*. This depicts a simple, closed-loop servomechanism with negative feedback. Although each investigator would alter the diagram slightly, few would reject its basic premise, that gives central importance to the respiratory center of the brain stem, which is assumed to react to changes in arterial blood. Most would also agree that the medullary center is under the partial influence of afferent impulse traffic from peripheral chemoreceptors in the arterial blood stream—the carotid and aortic bodies. Few would differ strongly with our position that this mechanism cannot explain the most important hyperpnea—that which regulates ventilation and maintains acid-base homeostasis during exercise and recovery.

Description of Unified Concept

The concept presented here is depicted diagrammatically in Fig. 1*B*. On comparing Figs. 1*A* and 1*B*, it is evident that there are two major differences between this concept and previously proposed theories of the respiratory control mechanism.

1) In addition to the known peripheral chemoreceptors which react to the P_{O_2}, P_{CO_2}, and H^+ (*9*) of arterial blood—the carotid and aortic bodies—an additional peripheral chemoreceptor mechanism is postulated whose activity is in some way related to the H^+ and P_{CO_2} of mixed venous blood.

A structure—the glomus pumonale—has recently been found in the wall of the pulmonary artery of several species, including man, by Krahl (*10*). These structures are identical microscopically with the carotid and aortic bodies, and it may be that these are the chemoreceptors of the mechanism postulated above. This is an attractive possibility because of the following embryologic and phylogenetic considerations: The carotid artery is derived from the third branchial arch; the aortic arch and brachiocephalic artery are derived from the fourth branchial arch; and the pulmonary artery is derived from the sixth arch. The carotid arteries and the aortic arch are known to have associated chemoreceptors; hence, it may be that the pulmonary artery also has an associated chemosensitive glomus.

2) The respiratory center in the medulla is no longer considered to be a sense organ which reacts to humoral agents; rather, it is analogous to a "computing mechanism" whose input is the afferent impulse traffic from the peripheral chemoreceptors and whose output is the efferent impulse traffic to the respiratory muscles.

The belief that the respiratory center has physiologically important chemosensitivity is more deeply rooted than almost any other concept in physiology. There is, however, little direct evidence to support this (*6*). While it can hardly be doubted that a respiratory center exists, the fact that hyperpnea is produced by inhalation of CO_2 does not necessarily mean that the respiratory center is being stimulated directly by the increased arterial P_{CO_2} resulting therefrom. Such an assumption is implicit, nevertheless, in most studies of the chemosensitivity of the respiratory center. To cite one of many examples, Hooker (*11*) set out to compare the effects of H^+ and P_{CO_2} on the respiratory

center. In his experiments he perfused the heads of dogs by way of the innominate artery and assumed that changes in respiratory activity reflected the results of stimulating the respiratory center chemically. Since the carotid arteries are branches of this vessel, it is now apparent that the observed ventilatory responses were mediated, at least partially, by the carotid bodies. But, since peripheral chemoreceptors were unknown in 1917, it did not occur to anyone that such experiments were evaluating anything other than the respiratory center.

A similar situation existed later, after the aortic and carotid bodies had been found and their major characteristics had been determined. To evaluate the respiratory center, Banus *et al.* (*12*) destroyed the known arterial chemoreceptors and related the ventilatory responses to the composition of arterial blood before and after intravenous injection of acidic solutions. They did not consider the possibility that a chemoreceptor mechanism might react to the composition of mixed venous blood, just as in 1917 physiologists did not consider the possibility of arterial chemoreceptors. It is certainly conceivable, therefore, that the observed changes in ventilation were due not to chemical stimulation of the medulla but, rather, to chemical stimulation of a chemoreceptor in the venous circulation.

There are also at least two a priori reasons for believing that the physiologically significant chemoreceptive mechanisms are peripheral: (i) In general, the central nervous system does not act as a sense organ in addition to functioning as a "computing mechanism." (ii) Proper functioning of the central nervous system, especially the brain, requires that the composition of its arterial blood supply be maintained within narrow limits. Since the arterial chemoreceptors are interposed between the lungs and the respiratory center, they are in a position to sense changes in arterial blood before this altered blood reaches the brain. They can thus initiate the ventilatory response necessary to maintain CO_2 and O_2 homeostasis in the brain's arterial blood supply.

Respiratory control mechanism considered as a servosystem. To return to Fig. 1*B*, which depicts the new concept, the medullary respiratory center is considered to receive afferent impulses in proportion to the stimulation provided the mixed venous blood chemoreceptor mechanism by the H^+ and P_{CO_2} of mixed venous blood, plus the stimulation given the carotid and aortic bodies by the H^+, P_{CO_2}, and P_{O_2} of arterial blood. The respiratory center reacts to this afferent impulse traffic from the peripheral receptors and sends efferent impulses to the respiratory muscles in proportion to the total afferent impulse traffic received. The energy expenditure and, concomitantly, the volume of breathing are regulated by this afferent impulse traffic.

The ventilatory response elicited by a given set of conditions in the arterial and mixed venous blood does not differ greatly from one normal individual to another. There are, however, numerous conditions in which the work of breathing is excessive. In these conditions, the response may be normal in terms of energy expended for breathing but impaired in terms of total ventilation (*13*).

The system depicted in Fig. 1*B* is a pair of closed-loop servomechanisms connected in parallel, each having a negative feedback. Stimulation of either peripheral receptor will tend to produce hyperventilation. But, since hyperventilation lowers the P_{CO_2} and H^+ of both arterial and mixed venous blood, both receptors will soon receive less stimulation, with a resultant tendency toward hypoventilation. Conversely, if the stimulation on either side is reduced, or if the transmission of impulse traffic is impaired, there will be a tendency toward hypoventilation. This results in an increase of H^+ and P_{CO_2} in both mixed venous and arterial blood as well as a reduction of P_{O_2} in arterial blood; thus, the tendency toward hypoventilation is almost neutralized. Because of influences such as these, the system tends to stabilize, since both loops are "closed" and each has negative feedback.

It is believed that the system shown is more stable than the less complex system of Fig. 1*A* for two reasons: (i) A pair of servomechanisms, connected in parallel, is inherently more stable than two servomechanisms connected in series. (ii) "Utilizing" the characteristics of both oxygenated and partially reduced blood results in greater stability than if oxygenated blood alone is "utilized." This is because the amphoteric hemoglobin molecule becomes a stronger base after releasing O_2, and its ability to buffer CO_2 and H^+ is increased thereby.

Postulating a chemoreceptor mechanism which is reactive to agents in mixed venous blood is not new. For many years Pi-Suñer (*14*) persisted in presenting his case for peripheral chemoreceptors in the lungs; Mitchell *et al.* (*15*) have recently suggested a receptor mechanism reacting to the CO_2 system in mixed venous blood; Schmidt (*7*) has considered the problem in detail; and the theoretical groundwork, in terms of the physical chemistry of blood, was laid 35 years ago by Hastings and Murray (*16*).

Development of Present Concept

The concept presented here was developed largely in an effort to explain the hyperpnea of exercise, which cannot be explained by any theory yet proposed. As a first step, the "whole picture" of exercise physiology, as developed by L. J. Henderson and his associates (*17*), was reconsidered. This resulted in appreciation of the fact that, although the composition of arterial blood in an exercising subject differs but slightly from the composition of arterial blood in the same subject at rest, the H^+ and P_{CO_2}, as well as the O_2 and CO_2 content, of mixed venous blood vary in relation to oxygen consumption. In fact, unless the physicochemical characteristics of mixed venous blood are considered in relation to $\dot{V}_{O_2}$, $\dot{Q}$, respiratory quotient, hemoglobin concentration, and so on, there is no "normal" value for any of the physicochemical variables in mixed venous blood.

These relationships are shown in Fig. 2, in which H^+ and P_{CO_2} of arterial and mixed venous blood, together with minute ventilation, are plotted against oxygen consumption. All of the points plotted were obtained by the indirect method (*18*) on normal men, as reported several years ago from the Harvard Fatigue Laboratory (*19–22*).

Cardiac output, $\dot{Q}$, as estimated by the "indirect" Fick method, simultaneously with the other variables, is also plotted. Although there is no direct relationship between the volume of cardiac output and the volume of breathing, an important indirect relationship deserves emphasis. Unless the cardiac output increases appropriately, as the consumption of O_2 and the production of CO_2 are increased, the normal relationship between the composition of arterial and of mixed venous blood will become deranged. Whenever there is

such a derangement, both arterial and mixed venous chemoreceptor mechanisms react accordingly, in the form of a deranged ventilatory response.

It is unnecessary to present values for HCO_3^-, since these can be calculated from H^+ and P_{CO_2}. Values for oxygen in arterial blood have not been plotted, since there is a great deal of evidence that this value changes little with any but extremely severe exercise (*15*, *23*, *24*). Values for O_2 in mixed venous blood have not been presented because the indirect method gives poor estimates of this variable and, also, because we have been unable to detect any generally consistent relationship between ventilation and the P_{O_2} in mixed venous blood.

Figure 2 shows, graphically, that for muscular exercise of any given intensity (definable in terms of increased O_2 consumption) there is an empirically definable change in the physicochemical characteristics of both mixed venous and arterial blood and, also, an increase in both ventilation and cardiac output. Figure 2 also presents equations which define these relationships as functions of $\dot{V}_{O_2}$. From this description of exercise we have inferred that the ventilatory response to exercise may be partially regulated, directly or indirectly, by the physicochemical characteristics of mixed venous blood.

Simulation of concomitants of exercise in experimental animals. In view of the striking relationship between the volume of pulmonary ventilation and the P_{CO_2} and H^+ of mixed venous blood, we attempted to determine whether experimental alterations of P_{CO_2} and H^+ of mixed venous blood would result in ventilatory changes that were independent of the characteristics of arterial blood.

We have not devised a method for perfusing the lesser circulation of a reasonably intact animal without altering the composition of blood in the systemic circulation. In the absence of such a method, the results of a study which allows separate *consideration* of the changes in the two bloods are presented in Fig. 3. This graph relates the hyperpneas resulting from electrically induced muscular exercise, intravenous injections of 1.0*M* lactic acid or sodium bicarbonate solutions, and inhalation of CO_2 to H^+ and P_{CO_2} of arterial and mixed venous blood. Since each procedure is associated with a characteristic $\dot{V} - H^+ - P_{CO_2}$ "profile," the effects of alterations in arterial and mixed venous blood can be considered separately.

The data are subject to the objection that they were obtained from anesthetized dogs lying supine, with varying degrees of acid-base imbalance and hypoxia. However, since the patterns are similar to prior observations [exercise, Henderson *et al.* (*17*, *19*); CO_2 inhalation, Hochrein *et al.* (*21*); metabolic acidosis (shock), Cournand

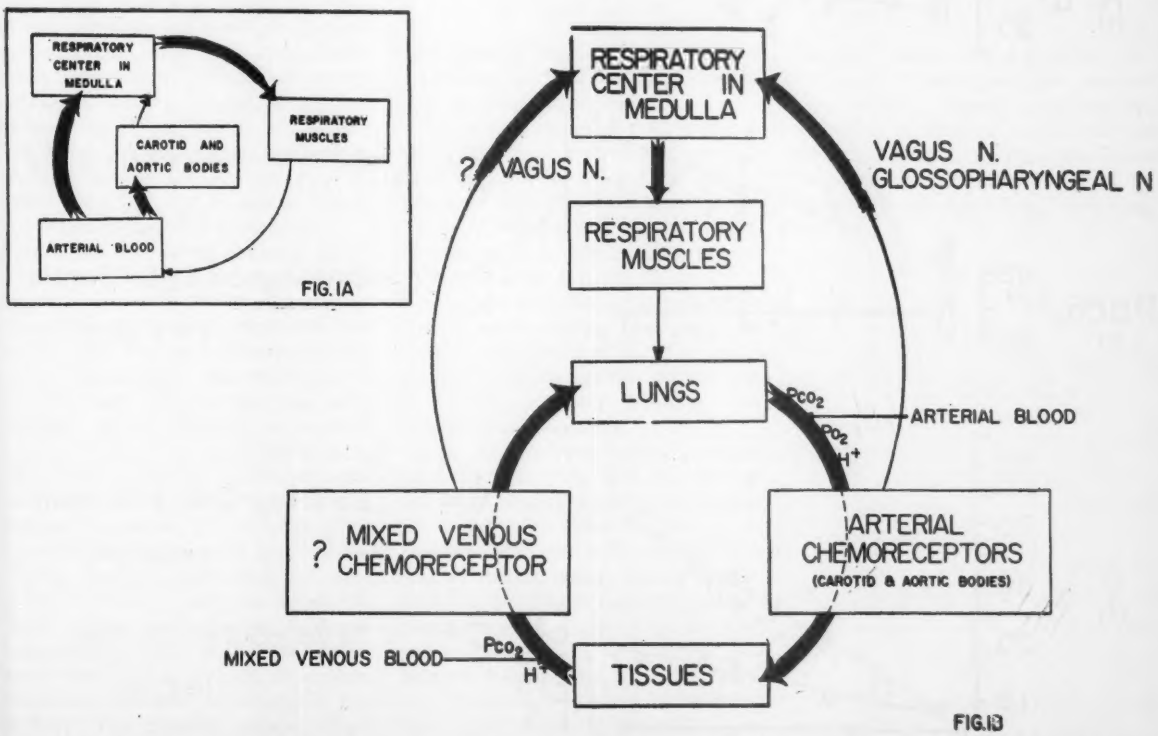

Fig. 1. (*A*) Diagrammatic representation of servomechanism analogous to classic theories of respiratory control mechanism. Arterial blood is thought to stimulate the peripheral arterial chemoreceptors and also the chemosensitive respiratory center; this, in turn, governs the activity of the respiratory muscles which ventilate the lungs and thereby regulate the composition of arterial blood. (*B*) Diagrammatic representation of servomechanism analogous to the present theory proposed to explain the regulation of breathing. In addition to the carotid and aortic bodies, which react to the H^+, P_{CO_2}, and P_{O_2} of arterial blood, it is postulated that there is another chemoreceptor mechanism which reacts to the H^+ and P_{CO_2} of mixed venous blood. The respiratory center receives impulse traffic from both receptors and governs the activity of the respiratory muscles accordingly. The respiratory center is believed to act as a computing mechanism whose functional integrity requires stable, rather than changing, composition of its perfusate.

et al. (*25*)] and since they fit the requirements of the O_2–CO_2 diagram (*26*), they are probably essentially correct.

These experiments are interpreted as follows.

1) Inhalation of CO_2, injection of lactic acid or sodium bicarbonate solution, and muscular exercise all produce hyperventilation, and each is associated with characteristic changes in P_{CO_2} and H^+ of both arterial and mixed venous blood.

2) The ventilatory responses to the four stimuli are not associated with parallel alterations of either agent in arterial blood; neither are they associated with parallel changes of either agent in mixed venous blood.

3) It may be possible to account for all of the responses by postulating that the volume of breathing is related to the *level* of both agents in *both* arterial and mixed venous blood. For instance, when sodium bicarbonate is injected, both arterial and mixed venous blood are made more alkaline and the P_{CO_2} is increased in both. Excluding the possibility that increased alkalinity, per se, produces hyperventilation, one might conclude that the stimulus is either P_{CO_2} of mixed venous blood or P_{CO_2} of arterial blood, or both. Yet, when hyperpnea is produced by giving lactic acid, the P_{CO_2} of mixed venous blood increases but the P_{CO_2} of arterial blood decreases. From consideration of the experiments with both bicarbonate and lactic acid, it might be concluded that

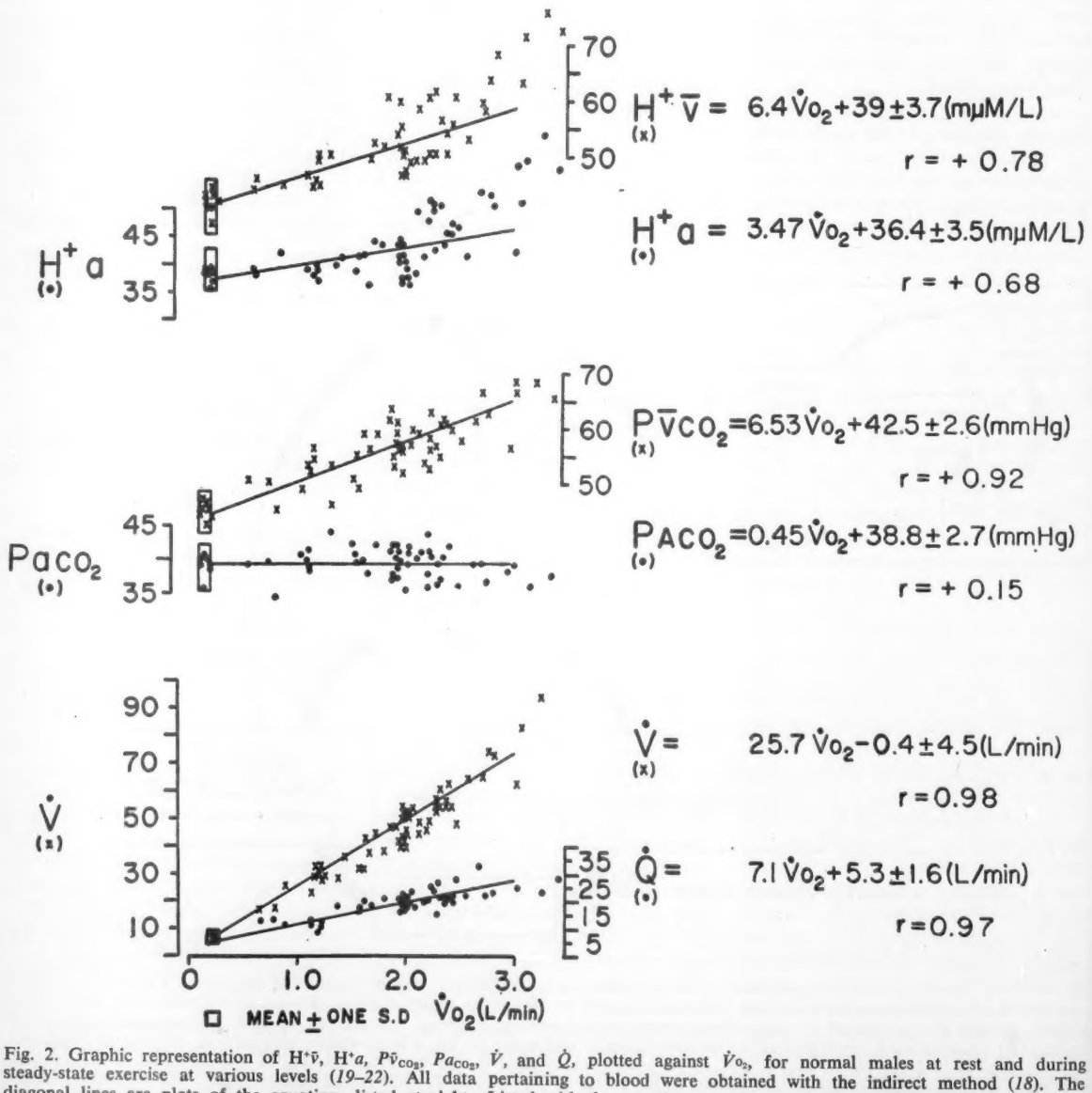

Fig. 2. Graphic representation of $H^+\bar{v}$, H^+a, $P\bar{v}_{CO_2}$, Pa_{CO_2}, $\dot{V}$, and $\dot{Q}$, plotted against $\dot{V}_{O_2}$, for normal males at rest and during steady-state exercise at various levels (*19–22*). All data pertaining to blood were obtained with the indirect method (*18*). The diagonal lines are plots of the equations listed at right. Listed with the equations are the correlation coefficients (r) between the dependent variables and $\dot{V}_{O_2}$, the independent variable. The standard error and the units used to express the dependent variable follow the ± sign.

P_{CO_2} of mixed venous blood is *the* stimulus, since it is *the* common factor. This would also be consistent with findings for exercise and for breathing CO_2, conditions in which $\dot{V}$ and $P\bar{v}_{CO_2}$ parallel each other. That this conclusion is not strictly valid is evident when the amount of hyperpnea per unit change of P_{CO_2} of mixed venous blood is compared in the different experiments; that is, the $\Delta\dot{V}:\Delta P\bar{v}_{CO_2}$ ratio was much less when $NaHCO_3^-$ was given than in the other experiments. [The conclusion is also inconsistent with the observation that in metabolic acidosis, including the state of recovery from exercise, the hyperventilation associated with a reduced $P\bar{v}_{CO_2}$ (*15, 25, 27*).]

It seems, therefore, that the hyperventilation attendant upon administration of bicarbonate is the result of a transient increase in P_{CO_2} of arterial and mixed venous blood—an increase whose influence temporarily exceeds the influence of simultaneous reduction of H^+.

To extend the interpretation of these results by considering, in turn, the ventilatory responses and blood gas changes in each of the other three experiments seems unnecessary. It was tentatively concluded that any general description of the humoral mechanisms which control breathing must give quantitative consideration to the H^+ and P_{CO_2} of mixed venous as well as arterial blood.

Ventilation and blood gases in subjects at rest and during exercise. Since it may be possible to account for the hyperpnea of exercise by considering the relationship between ventilation and the H^+ and P_{CO_2} of both arterial and mixed venous blood, data were needed to define the relationships that exist between these variables in normal human beings at rest and during exercise. The only known sources of such data are reports from the Harvard Fatigue Laboratory (*17, 19–22*) (Fig. 2). Values obtained with the indirect method in resting individuals are compared in Table 1 with the results of more recent studies in which direct measurements were used. The differences between several of the mean values are known to be statistically significant. This probably reflects differences between the subjects and the experimental situations as much as it reflects differences between the two methods. For example, for most of the variables listed in Table 1, values for recumbent subjects differ significantly from values for subjects who are erect. For this reason—since they are not physiologically significant—the results of statistical analyses have been omitted from the table deliberately.

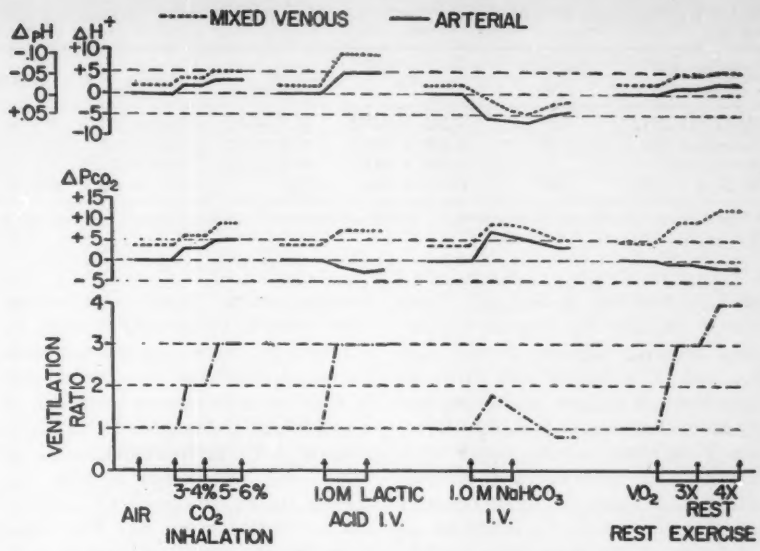

Fig. 3. Semidiagrammatic representation of changes in ventilation, Pa_{CO_2}, $P\bar{v}_{CO_2}$, H^+a, and $H^+\bar{v}$, resulting from muscular exercise, inhalation of CO_2, and intravenous administration of acidic as well as alkaline solutions. All experiments were performed on dogs lightly anesthetized with chloralose.

Results obtained with the two methods in exercising individuals are compared in Table 2. Since there are no reports of direct measurement of the P_{CO_2} or H^+ of mixed venous blood during exercise, it is only possible to compare the relation between O_2 consumption and the variables in arterial blood plus the cardiac-output response to increased consumption of O_2. The results obtained with the indirect method obviously compare very favorably with directly measured values.

Since the indirect method provided valid estimates of H^+ and P_{CO_2} of arterial blood during exercise, and since cardiac-output response to exercise, as calculated from the indirectly estimated P_{CO_2} of mixed venous blood, is very similar to the directly measured cardiac-output response, it follows that the relationships between O_2 consumption and both the P_{CO_2} and the H^+ of mixed venous blood, as shown and quantified in Fig. 2, are essentially correct.

The method used to analyze the data was as follows: O_2 consumption ($\dot{V}_{O_2}$) was assumed to be an independent variable; ventilation ($\dot{V}$), H^+a, $P\bar{v}_{CO_2}$, $H^+\bar{v}$, and Pa_{CO_2} were assumed to be dependent variables. Simultaneous equations, describing $\dot{V}$ in relation to H^+a, $P\bar{v}_{CO_2}$, and so on, were then solved so as to express $\dot{V}$ as a function of H^+a, $P\bar{v}_{CO_2}$ and so on. Standard statistical methods were utilized. Simple and multiple correlation coefficients so derived are shown in Table 3. Six regression equations, developed to define several of the relationships shown in Fig. 2, are listed below:

$$\dot{V} = 25.7\,\dot{V}_{O_2} - 0.4 \pm 4.5 \text{ lit./min} \quad (1)$$

$$\dot{Q} = 7.1\,\dot{V}_{O_2} + 5.3 \pm 1.6 \text{ lit./min} \quad (2)$$

$$Ca_{O_2} - C\bar{v}_{O_2} = \dot{V}_{O_2}/(7.1\,\dot{V}_{O_2} + 5.3) \text{ (cm}^3\text{/lit.)} \quad (3)$$

$$\dot{V}_{O_2} = 0.19H^+a - 0.12\,Pa_{CO_2} + 0.21\,P\bar{v}_{CO_2} - 0.16\,H^+\bar{v} - 4.61 \quad (4)$$

$$\dot{V} = 1.1\,H^+a + 2.3\,P\bar{v}_{CO_2} - 135 \pm 10.1 \text{ lit./min} \quad (5a)$$

$$VR = 0.17\,H^+a + 0.35\,P\bar{v}_{CO_2} - 21 \quad (5b)$$

$$VR = 5b + \frac{105}{10^{0.088P_{O_2}}} \quad (5c)$$

$$\dot{V} = 4.96\,H^+a - 3.2\,Pa_{CO_2} + 5.3\,P\bar{v}_{CO_2} - 4.2\,H^+\bar{v} - 123 \pm 6.6 \quad (6)$$

Equations *5a*, *5b*, and *5c* are of primary interest; their significance is discussed below in detail. Equation 1, which expresses the well-known relation between ventilation and consumption of O_2, is presented for complete-

Table 1. Comparison of data (means, plus or minus standard deviation) obtained by indirect (H F L) and direct methods with subjects at rest.

Author and reference	Subjects (N)	$\dot{V}_{O_2}$	$P_{O_2}\bar{v}$	Pa_{CO_2}	H^+a	$P\bar{v}_{CO_2}$	$H^+\bar{v}$	$\dot{Q}$
H F L (*19–21*)	18*	0.246 ± 0.032	41.3 ± 4.2	38.8 ± 2.4	38.7 ± 1.9	43.4 ± 3.8	40.4 ± 2.5	6.5 ± 1.1
Cournand (*28*)	11†	0.240 ± 0.033	39.4 ± 4.2	39.4 ± 3.8	37.9 ± 1.6	45.2 ± 4.4	41.3 ± 1.2	5.6 ± 1.2
Bartels (*38*)	9†	0.304 ± 0.047	39.4 ± 5.8	39.8 ± 2.0	36.1 ± 1.9	42.9 ± 2.1	38.0 ± 1.3	7.5 ± 1.1
M. S. & C. (*15*)	26‡	0.35 ± 0.06	35§	39.0 ± 4.0	40.8 ± 4.0	45§	43§	5.5 ± 0.9

* Eight subjects erect, ten subjects recumbent. † Subjects recumbent. ‡ Subjects erect. § Values calculated from CO_2 or O_2 dissociation curves constructed from $\dot{Q}$, Cao_2, $Caco_2$, $Paco_2$, R, $\dot{V}co_2$, Hb (*18*).

ness and need not be discussed. Equation 6 describes the empiric relationships observed between $\dot{V}$ and both P_{CO_2} and H^+ in arterial and mixed venous blood in subjects at rest and during exercise of various intensities. Equation 2 expresses cardiac output as a function of O_2 consumption as depicted in Fig. 2. Equation 3, which expresses arterio-mixed venous O_2 difference as a function of O_2 consumption, was obtained by combining Eq. 2 with the Fick equation [$\dot{V}_{O_2} = \dot{Q}\ (Ca_{O_2} - C\bar{v}_{O_2})$].

Many of the essential points that have been presented can now be highlighted and summarized by two sample calculations with Eq. *5a* that relate to a hypothetical normal man. The equations on the right side of Fig. 2 state that the H^+ of a resting individual's arterial blood will be about 38 mμmole per liter (pH, 7.42), and the P_{CO_2} of his mixed venous blood will be about 44 mm-Hg. Equation *5a* now becomes: $\dot{V} = (1.1 \times 38) + (2.3 \times 44) - 135 = 8$ lit./min. This is a reasonable value for a normal person at rest. During mild exercise, which will be assumed to require the consumption of 1.0 liter of O_2 per minute, the equations of Fig. 2 state that the H^+ in arterial blood will be increased from 38 to about 41 mμmole per liter, and the P_{CO_2} of mixed venous blood will be increased from 44 to about 50 mm-Hg. Under these conditions, Eq. *5a* predicts a minute ventilation of 25 liters per minute, agreeing with the value predicted by Eq. 1. In this example, the hyperpnea of exercise is accounted for by a change in the composition of both arterial and mixed venous blood with the latter seemingly the more important. Much of the remainder of this paper is based on similar, and equally simple, calculations.

Theoretically, Eq. *5a* will give less valid estimates of ventilation than Eq. 6. Equation *5a* is much easier to present graphically than Eq. 6, and we use it here to demonstrate the validity of the present concept. Equation *5b* was derived from *5a* by dividing all terms by 6.5 liters per minute (an assumed value for minute ventilation for a normal man at rest) so as to express ventilation as a multiple of the value for a subject at rest, or VR [the ventilation ratio of Gray (*8*)], rather than in liters per minute. In Eq. *5c* Gray's $VR_{P_{O_2}}$ factor has been added to Eq. *5b*. This factor quantifies the ventilatory response to hypoxia, isolated from the effect of Pa_{CO_2} *and* H^+a. Gray achieved this isolation by using PA_{CO_2} rather than $P\bar{v}_{CO_2}$, and his "$VR = 1$" referred to alveolar ventilation ratios, whereas we have used total ventilation. Hence, Eq. *5c* is inherently less true than if the coefficient for P_{O_2} had been calculated directly. It is presented only to show the results of using Eq. *5b* in hypoxic states.

Ventilatory response to exercise as a general mechanism. Next it was necessary to determine whether Eqs. 5 and 6, derived from indirect estimates of H^+ and P_{CO_2} during exercise, could predict ventilation in other normal subjects at rest and during exercise. It also remained to determine whether the equations provided a description of respiratory regulation sufficiently general to predict ventilation under other circumstances.

This problem may be analyzed graphically with the aid of Fig. 4, which is a Cartesian diagram of Eq. 5. In the figure, H^+a is on the horizontal axis, $P\bar{v}_{CO_2}$ is on the vertical axis, and ventilation is plotted with diagonal isopleths. The figure is used to compare the ventilatory response, as predicted and as determined, under various circumstances: rest (*28*), exercise (*15, 19–22*), inhalation of CO_2 (*29*), administration of sodium bicarbonate (*30*), administration of ammonium chloride (*31*), diabetic acidosis (*27*), shock (*25*), and recovery from exercise (*15, 32*).

Figure 4 confirms our interpretation of the findings presented in Fig. 3. It shows that if the characteristics of both arterial and mixed venous blood are dealt with quantitatively, the ventilatory responses to several types of acid-base imbalance follow rules similar to those that describe the ventilatory response to exercise.

Figure 4 also provides a new and semiquantitative explanation of several respiratory phenomena that are frequently explained by an assumed alteration of the sensitivity of the respiratory center or the peripheral chemoreceptors. For instance, during breathing of CO_2 the relation between ventilation and Pa_{CO_2} is at least as close as the relation between ventilatory response and the H^+ of the arterial blood. In these circumstances the respiratory control mechanism is considered to be equally

Table 2. Comparison of data obtained by direct (D)* and indirect (I)† methods during exercise. S.E., standard error of estimate; r, correlation coefficient.

Method	Observations (N)	Regression Eq.‡	S.E.	r	References
D	83	$H^+a = 36.7 + 2.9\ \dot{V}_{O_2}$ (1.5 to 4.3)	± 5.4 + 0.42		(*23, 39*)
I	56	$H^+a = 36.4 + 3.5\ \dot{V}_{O_2}$ (2.5 to 4.5)	± 3.5 + 0.68		(*19–21*)
D	83	$Paco_2 = 39.9 - 0.60\ \dot{V}_{O_2}$ (−0.16 to + 0.43)	± 3.9 − 0.13		(*23, 39*)
I	56	$P_{ACO_2} = 38.8 + 0.45\ \dot{V}_{O_2}$ (−0.34 to + 1.24)	± 2.7 + 0.15		(*19–21*)
D	271	$\dot{Q} = 5.6 + 6.5\ \dot{V}_{O_2}$ (6.2 to 6.8)	± 1.2 + 0.94		(*40*)
I	56	$\dot{Q} = 5.3 + 7.1\ \dot{V}_{O_2}$ (6.5 to 7.7)	± 1.6 + 0.97		(*19–21*)

* Range of work, $\dot{V}_{O_2}$ 0.11 to 3.0 lit./min. † Range of work, $\dot{V}_{O_2}$ 0.21 to 3.45 lit./min. ‡ Values in parentheses, fiducial limits ($P = .05$) of regression coefficient.

sensitive to P_{CO_2} and to H^+. However, in the situation of metabolic acidosis [in the lactic acid injection experiments of Fig. 3, in shock (*25*), in diabetic acidosis (*27*), and during recovery from exercise (*15*)], there is hyperpnea even though the P_{CO_2} of the arterial blood drops below normal. Thus, it might be concluded that the mechanism has become less sensitive to P_{CO_2} than to H^+. Likewise, the hyperpnea of exercise is frequently explained by an alleged increase of the mechanism's sensitivity to both agents. This argument requires that there be a change in "sensitivity" at the beginning and end of exercise.

It is preferable to account for the change in sensitivity with a different argument—that ventilatory response is determined by the *resultant* of chemoreceptor influences in arterial and mixed venous blood, these influences being dependent on the physicochemical characteristics of their respective blood supplies. Specifically, then, during breathing of CO_2 the H^+ and P_{CO_2}, of both arterial and mixed venous blood are increased; the same is true during exercise except that the Pa_{CO_2} does not rise. In either case, both chemoreceptors are stimulated. The resultant curve, as plotted in Fig. 5, is nearly perpendicular to the isoventilation lines, and a large ventilatory response is to be expected. In situations of metabolic acidosis the HCO_3^- is decreased—by the reaction:

$$HA + NaHCO_3^- \rightarrow NaA + H_2O + CO_2 \uparrow$$

—the CO_2 being exhaled. The differences between the H^+ and the P_{CO_2} of arterial and of mixed venous blood must now increase, and the mixed venous blood will be considerably more acidotic and hypercarbic than the arterial blood. The resultant of these influences nearly parallels the isoventilation lines, and mild hyperpnea is to be expected.

It seems evident that Eqs. *5a*, *5b*, and *5c* are more than a description of the relationship between ventilation and alterations in the H^+ and P_{CO_2} of arterial and mixed venous blood in a subject at rest and during exercise. They also predict the ventilatory response to several types of acid-base imbalance with acceptable accuracy.

It was obviously necessary to determine whether Eq. 5 could be improved upon by considering the H^+ and the P_{CO_2} of arterial and of mixed venous blood together. This resulted in Eq. 6. Values for ventilation, as predicted with the four independent variables of Eq. 6, were not appreciably different from the values predicted with the two independent variables of Eq. 5. This is not surprising in view of the variability inherent in the data, plus the known deficiencies of our procedure. It was assumed, for instance, that there were linear relationships between the variables. Inspection of Fig. 2 shows that this is not the case, especially for the $\dot{V}_{O_2}$—H^+a relationship. In the development of Eq. 5, only three such assumptions are made; in Eq. 6 it is assumed that nine relationships are linear. It seems probable, therefore, that adding two more variables, $H^+\bar{v}$ and Pa_{CO_2}, introduced at least as much error as was removed.

Table 3. Statistical analysis of data from the Harvard Fatigue Laboratory reports, obtained by the indirect method (*18–22*).

	Simple and multiple correlation coefficients					
	$\dot{V}$	$\dot{V}_{O_2}$	Pa_{CO_2}	H^+a	$P\bar{v}_{CO_2}$	$H^+\bar{v}$
$\dot{V}$	1					
$\dot{V}_{O_2}$	+0.98	1				
Pa_{CO_2}	+0.17	+0.15	1			
H^+a	+0.70	+0.68	+0.10	1		
$P\bar{v}_{CO_2}$	+0.88	+0.83	+0.43	+0.62	1	
$H^+\bar{v}$	+0.82	+0.78	+0.31	+0.92	+0.85	1
$\dot{Q}$		+0.97				
$H^+a \cdot P\bar{v}_{CO_2}$	0.92					
$H^+a \cdot Pa_{CO_2} \cdot H^+\bar{v} \cdot P\bar{v}_{CO_2}$	0.98					
$Pa_{CO_2} \cdot H^+\bar{v}$	0.82					

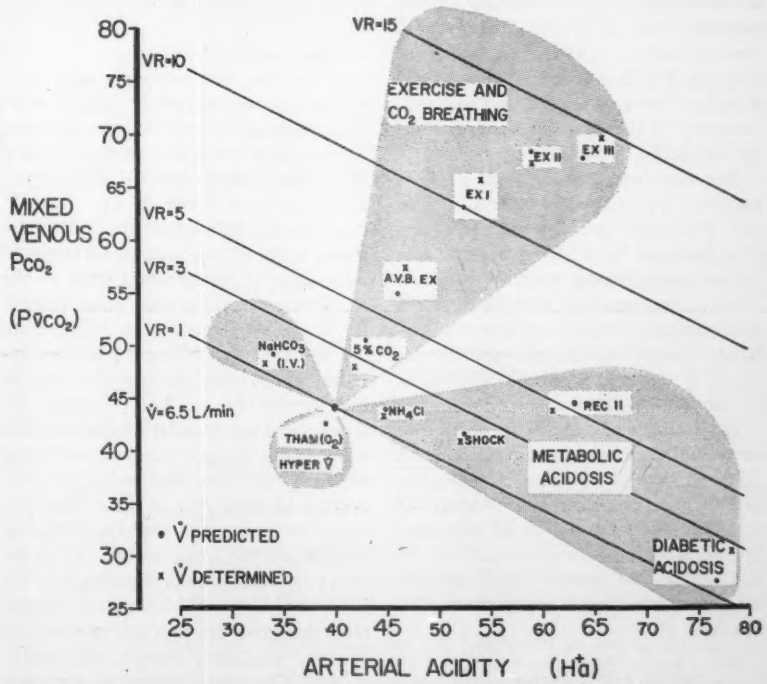

Fig. 4. Application of Eq. *5a* or *5b* in normal persons during exercise, during recovery from exercise, and in several situations characterized by acid-base imbalance. Diagonal lines represent isoventilation isopleths. Values for ventilation, as determined (×) and as predicted by Eq. 5 (•) in several conditions, are shown. The points were obtained from sources referred to in the text.

Present Concept and Multiple-Factor Theory

Equation 5*b* [$VR = 0.17\ H^+a + 0.35\ P\bar{v}_{CO_2} - 21$], which was derived from Eq. 5*a* by dividing each term by an assumed value of 6.5 liters per minute for the ventilation of a normal man at rest, is strikingly similar to the equation presented by Gray (*8*) to quantify his multiple-factor theory:

$$VR_{H^+P_{CO_2}} = 0.22\ H^+ + 0.26\ P_{CO_2} - 18 \quad (7)$$

This equation is the best available quantitative description of the ventilatory response of normal resting individuals to alterations in acidity and CO_2 tension in *arterial* blood. If it is to be used in conditions where arterial hypoxia exists, a third term, $105/10^{0.038P_{O_2}}$, is added.

Our equations are compared with Gray's in Fig. 5, which shows the ventilatory responses to acute and chronic hypoxia (*33*) in addition to responses in several of the situations presented in Fig. 4.

Although neither Gray nor his colleagues (*4*) have claimed that the multiple-factor theory explained the hyperpnea of exercise, it probably gives a valid estimate of the amount of hyperpnea due to activity of *arterial* chemoreceptors. Thus, the exercise point in Fig. 5 shows that the arterial stimulus accounted for about two-sevenths of the exercise hyperpnea of the subject (A.V.B.) (*17*).

It is surprising that Eq. 5*b* and Gray's equation are so similar, especially since our equations were developed from data obtained by studying normal men at rest and during exercise, whereas Gray's equation was developed from data collected from a completely different group—resting persons breathing CO_2 or with metabolic alterations of acid-base balance.

A partial explanation for this similarity is that identical assumptions were made in both derivations. Gray's procedure was essentially as follows. If ventilation is assumed to be regulated by the algebraic summation of the H^+ and the P_{CO_2} of arterial blood, the following equation holds in the absence of hypoxia:

$$VR = a\,H^+ + b\,P_{CO_2} - c$$

The coefficients and the intercept were derived by solving simultaneous equations which described the buffer systems of blood, alveolar ventilation as a function of both Pa_{CO_2} and H^+a, and the empirical relationship between Pa_{CO_2} and H^+a. An identical assumption that there is algebraic summation is also made when a multiple regression equation is derived by the least squares method; hence, it is not surprising that Gray's equation resembles Eq. 5*b* in *form*.

If the Pa_{CO_2} *in* Gray's equation is changed to $P\bar{v}_{CO_2}$, it will be found that the intercept becomes 20 rather than 18. This modification of Gray's equation

$$VR_{H^+a\ P\bar{v}_{CO_2}} = 0.22\ H^+a + 0.26\ P\bar{v}_{CO_2} - 20$$

predicts the ventilatory response to exercise and to acid-base imbalance in resting persons nearly as well as Eqs. 5*b* or 7. The similarity between these equations is interpreted to mean that both describe a general physiologic reaction pattern.

Unified Concept Applied to Cross-Perfusion Experiments

If this concept is valid, it should provide an explanation for the results observed by Kao (*34*) in his studies of respiratory regulation in cross-perfused dogs.

These were of two types.

1) In one, the two dogs were connected, jugular veins to jugular veins and common carotid arteries to common carotid arteries. Arterial blood from the central ends of the carotid arteries of the donor (Kao's "neural" dog) passed through *T* tubes into the distal ends of the carotid arteries of both dogs. The proximal ends of the recipient's carotid arteries were ligated. By a similar arrangement, blood from the head of the recipient (Kao's "humoral" dog) was returned by way of the jugular veins. The vertebral arteries and neck muscles of the recipient were ligated so as to separate the blood supply of the head and body. It is important to note that in both dogs the anastomosis was interposed between the carotid and the aortic bodies. Thus, the recipient had a humoral connection between his body and his aortic chemoreceptors, but no humoral connection to his respiratory center or carotid bodies. The neural pathways were not interrupted in either dog.

When the donor was made to exercise by means of electrical stimulation, the donor manifested hyperventilation but the recipient did not. We interpret these findings as follows. (i) The donor dog was normal except for the aforementioned vascular anastomosis, and hyperventilation should occur with exercise if $\dot{V}$ is mediated by chemoreceptors responding to the composition of mixed venous blood. (ii) However, if the arterial blood had carried the stimulus, the recipient would also be expected to manifest hyperventilation. The absence of hyperventilation in the recipient indicates that the arterial blood was not providing an important stimulus—to either the medullary respiratory center or the carotid bodies.

When the recipient was made to exercise, hyperventilation occurred in the recipient only. We interpret these findings as follows. (i) Since the arterial blood did not contain the necessary stimulus, hyperpnea in the recipient cannot be accounted for on the basis of aortic-body stimulation. (ii) However, during exercise, the mixed venous blood of the recipient was undoubtedly hypercarbic and hyperacidic and, by acting on a chemoreceptor, could produce the observed hyperpnea.

2) In another series of experiments, the anastomosis was established by connecting common iliac artery with common iliac artery and common iliac vein with common iliac vein. In the donor dog, these vessels were ligated above the anastomosis; in the recipient, below the anastomosis. Thus, except for a small amount of collateral circulation, all blood draining the legs of the donor passed into the body of the recipient.

When the donor was exercised, the recipient manifested hyperventilation; the donor did not. It can be assumed that the venous blood draining the exercising legs of the donor was more acidic and hypercapneic than the recipient's resting blood; this is thought to have stimulated the mixed venous chemoreceptor mechanisms of the recipient and to have produced hyperventilation (*35*).

Indices of Chemoreceptor Activity and Respiratory Drive

Throughout previous sections of this article reference has been made to P_{CO_2} and H^+ as if they were the agents which stimulate the chemoreceptors and thereby regulate breathing. They should probably be considered *indices* of stimulation rather than agents which stimulate. Although the nature of the

stimulus actually responsible for action potential activity in the carotid and aortic bodies is not known, it is reasonably certain that acetylcholine and choline esterase are involved (*5, 36*). Whatever the substances responsible for this activity may be, there is fairly good evidence that the reactions are governed, indirectly, by the composition of the fluid perfusing the glomera.

For instance, Witzleb *et al.* (*37*) have observed a direct relation between the frequency of action potential discharge in the sinus nerve and the H^+ or P_{CO_2}, but an inverse relationship with the P_{O_2}, of fluid perfusing the carotid body. The approximately linear relation between action potential discharge and either H^+ or P_{CO_2} in these studies is paralleled by a similar relation between either the P_{CO_2} or the H^+ of arterial blood and ventilatory response, reported by Gray (*8*). The exponential increase in action potential frequency as the P_{O_2} of the perfusate is decreased (*37*) is also paralleled by an exponential increase in ventilation in relation to P_{O_2} of arterial blood when the influence of P_{CO_2} and H^+a is eliminated mathematically (*8*). It seems, therefore, that either ventilatory response or chemoreceptor activity can be estimated by measuring the H^+, P_{CO_2}, or P_{O_2} of the perfusate and using them as *indices* with which chemoreceptor activity or ventilatory drive can be monitored.

It is not supposed that the chemoreceptors are sensitive to each of these agents; rather, it is generally believed that their effect is produced by a common factor—possibly by alteration of the intracellular acidity of the glomeral cells.

This again brings up the important point of changes in the sensitivity of the respiratory control mechanism. It is known, for example, that after acclimatization to high altitudes the mechanism becomes more sensitive, in that the ventilatory response to a given amount of exercise is greater at altitudes than at sea level. A similar situation exists in anemia. What feature is common to both conditions? It is believed that the explanation lies in the reduced buffering power of blood. After acclimatization, the HCO_3^- is reduced; in anemia, the hemoglobin concentration is reduced. In either case the buffering power of the blood is reduced. Consequently, addition of a given volume of CO_2 or acid produces greater alteration of H^+ and P_{CO_2} in the mixed venous blood than if the hemoglobin or HCO_3^- concentrations are normal. This increased "sensitivity" of the blood buffer system is probably reflected in a simultaneously increased lability of the H^+ within chemoreceptor cells.

When using Eq. 5 we have taken H^+ concentration as an index for monitoring the entire physicochemical system of arterial blood, which partially governs the intracellular acidity of the arterial chemoreceptors. The P_{CO_2} has been taken similarly as an index in the case of mixed venous blood. Because of this, it may appear that we attach undue importance to the P_{CO_2} of mixed venous blood. To refute this, another equation is presented which accounts for the various ventilatory responses nearly as well as Eq. 5. This other equation was derived from the same data but uses the H^+ of mixed venous blood and the P_{CO_2} of arterial blood:

$$\dot{V} = 0.85\ H^+\bar{v} + 0.10\ Pa_{CO_2} - 5.2 \quad (8)$$

$$r\dot{V} \ldots .H^+\bar{v} \cdot P_{ACO_2} = 0.82$$

(S.E., ± 11.5 lit./min)

When applied to the group-1 exercising subjects of Mitchell *et al.* (*15*), this equation predicts a $\dot{V}$ of 55; the measured $\dot{V}$ was 73 liters per minute.

Thus, Eq. 8 uses the H^+ of mixed venous blood as an index of the activity of the mixed venous chemoreceptor mechanism and the P_{CO_2} of arterial blood as an index of carotid- and aortic-body activity.

Rather than use H^+a and $P\bar{v}_{CO_2}$ as indices, it would be preferable, of course, to estimate intracellular acidity from P_{CO_2}, H^+, HCO_3^-, and other blood buffers. We have not yet used this approach, principally because the data obtained with the indirect method do not justify the massive computation that would be required.

Summary

The clasic concept that breathing is regulated only by chemoreceptor mechanisms in the arterial blood stream—the respiratory center, the carotid and aortic bodies—has been modified in two ways. It has been expanded by postulating another chemoreceptor which reacts to the composition of mixed venous blood. It has been qualified by questioning the assumption that the respiratory center has physiologically significant chemosensitivity. (The respiratory center is considered to be, on the contrary, primarily a computing mechanism that integrates information received from chemoreceptors responding to both arterial and mixed venous blood.)

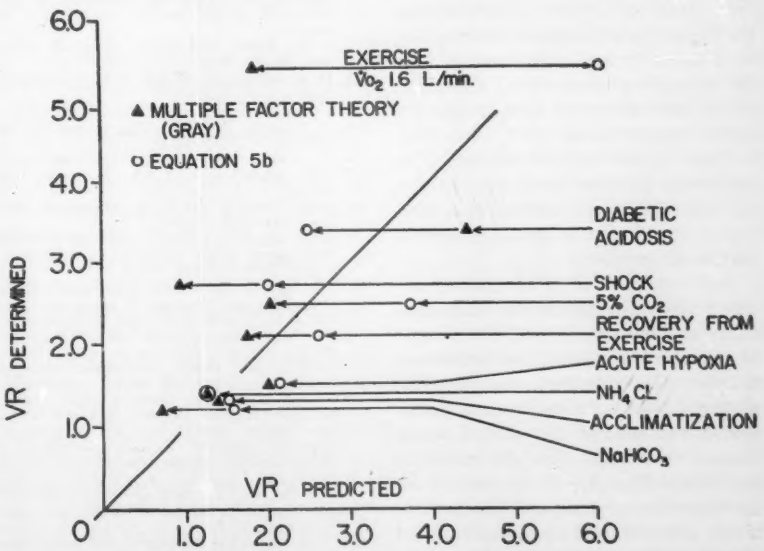

Fig. 5. Correlation of *VR* (determined) and *VR* (predicted) in several conditions—a comparison of Eq. 5 and the multiple-factor theory equation of Gray. *VR* (determined) is on the vertical axis, and *VR* (predicted) is on the horizontal axis. Since the scales are identical, perfect agreement is represented by the 45° diagonal. The graph shows that Eq. 5 and the multiple-factor theory equation have about equal ability to predict the various ventilatory responses of resting individuals. It also shows the approximate magnitude of the arterial stimulus, as estimated from Pa_{CO_2} and H^+a during exercise.

By postulating a chemoreceptor mechanism which reacts to the composition of mixed venous blood, a variety of ventilatory responses can be accounted for with a unified and semiquantitative concept based on the ventilatory response to exercise.

For the quantitative description of exercise, data obtained from normal men at rest and during exercise have been used to develop several equations which describe the ventilatory response to exercise in terms of P_{CO_2} and H^+ of arterial and mixed venous blood. The simplest of these equations, yet a useful one, is the following:

$$\dot{V} = 1.1\ H^+a + 2.3\ P\bar{v}_{CO_2} - 135 \quad (5a)$$

which states that the volume of breathing is determined by the algebraic summation of influences in arterial and mixed venous blood.

Ventilatory responses, as measured in several experimental and clinical situations characterized by acid-base imbalance and associated hyperpnea, have been compared with the ventilation predicted by this equation and by the equation which quantifies Gray's multiple-factor theory. Equation 5*a* estimated the various ventilatory responses as closely as the multiple-factor theory equation did. Equation 5*a* was also able to predict ventilation during exercise. It is concluded, therefore, that the hyperpnea of muscular exercise may be a generally applicable expression of the ventilatory response to alterations of the composition of both arterial and mixed venous blood.

When applied to the ventilatory responses of cross-perfused animals during exercise, the concept gives a satisfactory qualitative explanation of the various observations.

Although there is strong indirect evidence that a chemoreceptor mechanism exists which reacts to the composition of mixed venous blood and whose activity can be quantified, the equations that have been developed are not definitive expressions of the stimuli which regulate breathing. That the equations apply as well as they do, in view of the known errors of fact in their development, is probably the best evidence yet adduced for the essential validity of the present concept.

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Science in the News

Hanford and Stanford: $100 Million Project Approved; The PRDC Case: Private Safety and Public Power

The Joint Committee on Atomic Energy has approved the Administration's requests for $114 million to build the long-pending Stanford linear accelerator, and for $97 million to convert the Hanford plutonium reactor to produce electric power as well as plutonium. Both projects have been involved in political complications, and although they have nothing to do with one another, they had become tied together in the course of the political maneuvering.

The Stanford accelerator will be the most powerful atom-smasher in the world, and the most expensive. It will be housed in a tunnel 2 miles long near the Stanford campus, and will deliver a beam of electrons, carrying energies of up to 15 billion electron volts, to the target end of the tunnel. At some future time the machine could be stepped up to 45 billion electron volts. (A more detailed description of the machine and discussion of the political background appeared in *Science* for 22 April 1960.)

The so-called New Production Reactor (NPR) was approved by Congress in 1958 to provide additional plutonium for weapons. The Democrats, who controlled Congress and the Joint Committee, wanted a dual-purpose reactor to produce power for the federally owned Bonneville Power Administration, as well as plutonium. Bonneville is the Northwest's equivalent of TVA. The Eisenhower Administration, which took a dim view of public power, favored a single-purpose reactor. It was not clear at the time whether a dual-purpose reactor would be economically sound—that is, whether it would be cheaper to build a single-purpose reactor plus conventional power plants, which might be privately owned, or to build the dual-purpose plutonium-and-power reactor.

Over the opposition of the Administration the Joint Committee recommended—and Congress, after a floor fight, approved—an appropriation of $145 million for the NPR, of which $25 million represented the additional cost of designing and building the reactor so that it could be converted to dual-purpose operation. To actually convert the plant would cost about another $100 million, most of the money to be spent for generating equipment, and the decision as to whether conversion should be carried out would depend on the results of futher economic studies. In other words, the Democratic Congress was willing to gamble $25 million that the dual-purpose reactor would prove to be economically sound, as preliminary studies indicated, and the Administration, which was opposed to public power, was not so willing.

The Administration's opposition to public power heightened, in fact, the Democrats' interest in this particular proposal. During the Eisenhower Administration expansion of public power had virtually come to a halt, for although the Democrats had control of Congress for the last six Eisenhower years, they did not have a majority to pass a public power appropriation over the President's veto. In any case, Congress can only make money available; it cannot force the Administration to spend it. Here was a chance for the Democrats to force the Administration to move forward on public power whether it liked it or not, for the Administration wanted the new plutonium facility and Congress had specifically authorized only one type of reactor: the one convertible to power production. The extra $25 million would go mostly to pay for a more elaborate cooling system for the reactor, in which steam would be produced, which would then be cooled in the Columbia River. The cheaper alternative was to use the river water to cool the reactor directly. The indirect cooling system provided the option of using the steam to drive generating turbines.

The original studies prepared by General Electric, the contractor, suggested that the dual-purpose reactor would probably, although not clearly, be economically sound. This report came in April 1958 and provided the basis for investing the extra $25 million. After the money was voted, a second study, prepared for the AEC by an independent engineering firm, stated that the proposal was unsound. A year later, a study made by the Federal Power Commission concluded that it might pay to go through with the conversion, and then again it might not. The Joint Committee, which had less than complete faith in the Eisenhower Administration's estimates on questions involving public power, meanwhile had hired its own firm of consulting engineers and in August 1960 received a report confirming its view that the project was sound after all.

By this time the Stanford reactor had become involved in the picture, with strong voices on the Joint Committee encouraging the Administration to show more interest in Hanford if it wanted cooperation on Stanford and other AEC problems.

The Administration agreed to produce an updated FPC study of the feasibility of the Hanford project. In November 1960, after the election, the Joint Committee asked the Atomic Energy Commission for another review. The conclusion of both reports, submitted after the change in administration, was that the conversion to power would be clearly sound.

Party-Line Vote

Spokesmen for private power companies challenged this view, but the impression left by public hearings on the question several weeks ago was that there was indeed a sound basis for going ahead with the power generators, for even the minority members of the committee most strongly opposed to the project did not seriously challenge the favorable reports. There was a question as to how profitable the generating plant would be. A case could could be made for the view that the gains would be only marginal, and on this basis there was still opposition from opponents of public power. But there was never much doubt as to how the issue

was going to be resolved in the committee. The vote this week, on a nearly straight party line, endorsed the proposal, and although the bill faced a floor fight in the House, the project's supporters seemed fairly confident that the Administration would win.

At the same time the Joint Committee voted to support the full $114 million authorization for the Stanford accelerator; this the committee had refused to do last year on the basis of a number of mostly trivial reasons, one of them being resentment over the Eisenhower Administration's lack of sympathy for the Hanford project. What the committee did last year was to allow the Stanford project enough money ($3 million to go ahead to the point of actually beginning to move dirt. No one is quite sure how much this delayed the project; at the time the decision was made it was not completely clear, in fact, that the limited authorization would result in any delay at all, although it now seems that it may have cost six months. The committee saw no great harm in granting the project only a limited authorization last year, and it therefore allowed itself to be swayed by factors that would have had little or no influence had the life of the project actually been at stake. These included the official reasons cited by the committee—principally the desire to impress on the AEC the need for the best possible cost estimates, since in the past Congress had authorized money for accelerators only to learn later that the actual cost was going to be considerably larger than the original estimates. There were also unofficial reasons, such as the feeling that it might be just as well to delay final authorization until this year, when there might be a Democratic administration to take credit for the project, and a disinclination to provide Nixon with the occasion for dramatizing the Republicans' interest in science at a groundbreaking ceremony in the midst of the election campaign. Now the political campaign is over; both the White House and Congress are in the hands of the same party; and both Hanford and Stanford seem pretty solidly established.

The PRDC Case

Public versus private power was also an issue in another controversy involving, ostensibly, only the Atomic Energy Commission's handling of reactor safety problems. This week the Supreme Court handed down a decision which upheld the Commission's procedures for issuing construction licenses for large power reactors; these so-called developmental reactors always include a number of untried features, since at this stage in the development of atomic power there is no point in building a power reactor unless it incorporates substantial changes over earlier reactors. The case was described here in some detail on 5 May, at the time it was argued before the court.

The AEC uses a "two-step" procedure in dealing with these reactors: it issues a construction permit after it is satisfied that, among other things, there are no safety problems that are not likely to be resolved during the construction period. The company can then go ahead with advanced design and construction with the assurance that if the finished reactor meets the AEC safety standard it will be granted an operating license. The AEC standard is that there must be "no credible possibility of an accident that will release significant quantities of fission products" into the air. The decision to allow operation can be appealed to the courts if anyone who might be harmed by the reactor feels the AEC has acted improperly.

A group of unions challenged this two-step procedure. They said that once the AEC had permitted a corporation to invest perhaps $50 million in a reactor it would be under heavy pressure to let the reactor operate even if it fell short of the strict safety standard. Because of the danger of this influence, the unions felt that the same safety standard should be required when a construction permit is issued as would be required for the operating permit. Although this argument was brought to the attention of the courts, as well as the argument that reactors are so dangerous that extraordinary precautions are necessary to protect the public, the legal basis for the case had to be something different. For there is little legal basis for arguing that a construction permit cannot be issued because the AEC might behave illegally when the time came to issue an operating license, and equally little legal basis for arguing that the courts ought to take over the function of Congress and decide what public policy should be on questions of reactor safety.

For their main argument, then, the unions had to work out a line of reasoning to support a view that the Atomic Energy Act did not permit the AEC to use the two-step licensing procedure, and then to hope that the court would be sufficiently impressed by the dangers of reactors to, in effect, rewrite the law to fit the unions' view of what public policy ought to be.

Justices Black and Douglas found the unions' view convincing. Speaking for the two dissenters, Douglas called atomic reactions "the most awesome, the most deadly, the most dangerous process that man has ever conceived." Against this background, the two justices accepted the union's interpretation of the law.

The other seven justices saw "good reason" for the AEC's two-step procedure, and no basis for the unions' view that the procedure is illegal.

Meanwhile, the question of public versus private power was mentioned nowhere in the argument over the case, since it had nothing to do with the legal issues. Nevertheless, it was a central factor and perhaps the central factor in the entire affair.

If the unions' view was upheld it would pretty much end, for the time being anyway, the participation of private power interests in the development of atomic power: no one would be interested in building a large power reactor unless it incorporated substantial new design features, and it would be very difficult for such a reactor to meet the strict safety requirements for a preliminary permit unless it were located so far from population centers that it would have little market for its power. All power reactors now lose money (the Hanford plant will be an exception only because the power is a by-product of plutonium production), but plants which could not recoup a good part of their cost by selling power would lose a great deal more money. Probably only the government could afford to build them.

This does not mean that the AEC's critics are not genuinely concerned over the dangers of nuclear reactors. But it is doubtful that the case ever would have been fought if the almost equally emotion-charged issue of public versus private development of atomic power had not been deeply involved. Much of the strong feeling about the case stemmed from a conviction that the public was being put in danger in order to make it possible for private power interests to take over atomic power, a situation which, if true, would strike supporters of public power as especially outrageous after the possibilities of atomic energy had been developed only through vast public expense.—H.M.

Rockets, Large and Small

Administration officials have described their ideas on a rocket to send a manned capsule to the moon. Space Agency officials told the Senate Science and Aeronautics Committee that the "Nova" rocket would stand about 120 yards high, that is the length of a football field, including the end zones. The first ("booster") stage would consist of eight rockets, each producing 1.5 million pounds of thrust. The second stage would use two such rockets. These would be topped by three further stages, requiring progressively small rocket clusters. The payload, of course, would have to include a rocket to bring the manned capsule back from the moon, but this would require only 1/500 the power of the original rocket, both because of the smaller gravitational pull by the moon and because the capsule would quite quickly reach a point where it would naturally fall toward the earth, rather than back toward the moon.

At the other end of the rocket spectrum, the Army last week demonstrated a "rocket belt" strapped on the back of a man. Harold Graham, a test engineer for Bell Aerosystems, which developed the rocket, made a 14-second demonstration journey in which he floated up 15 feet, sailed over a truck, and at about 20 miles an hour, was wafted to a landing about 50 yards away.

Announcements

Writing in the natural sciences and their engineering and technological applications, exclusive of the field of medicine, are eligible for the 1961 **AAAS Westinghouse Science Writing Competition**. The two $1000 awards, one for magazine writing and one for newspaper writing, will be presented at the annual dinner of the National Association of Science Writers on 27 December in Denver, Colorado. A magazine article or a newspaper or press association report, exclusive of those appearing in trade journals or professional scientific magazines, must have appeared in publications within the United States between 1 October 1960 and 30 September 1961 in order to be eligible. Each entrant may submit for consideration as many as three separate articles or series of articles published during the contest year. Persons other than the author may nominate entries. Deadline for all entries is *10 October 1961*.

In addition to the cash awards, citations will be awarded the newspaper and magazine in which the winning articles appeared. Honorable mention citations or other special recognition for distinguished service to science journalism will be made at the discretion of the judges. (Graham DuShane, AAAS, 1515 Massachusetts Ave., NW, Washington 5, D.C.)

A fishery research vessel is being built under the new national oceanographic program in which concerted attention will be given to the national effort in basic and applied ocean research. The vessel was designed by biologists and naval architects to meet the needs set forth by the fishery scientists of the Department of the Interior's Bureau of Commercial Fisheries. Its task will be to perform the numerous studies necessary to determine the distribution and variation in abundance of fish in the bottom Northwest Atlantic; to conduct various phases of oceanographic research in water temperature and factors affecting plant and animal life in the sea; and to obtain information on surface and subsurface water movements. The ship will cost $1,775,000, including basic laboratory and research facilities. It is scheduled to begin operating out of the Woods Hole Oceanographic Institution on Cape Cod in approximately 15 months.

A high-speed **electronic computer**, reported by the Atomic Energy Commission to be the largest and fastest situated on a university campus in this country, has been placed in operation at Rice University. The prime objective of the machine, now being used by the university's scientists and engineers, is to handle computations in theoretical physics and chemistry of particular interest to the Atomic Energy Commission's physical research program. It is also being used as a research tool in exploring ways to improve computers. It is anticipated that in the near future the machine will be made available to scientific personnel engaged in fundamental research in the Southwest.

Half of the cost of assembling the computer, approximately $250,000, was provided by the AEC; the remainder was provided from university sources.

The fifth volume in a continuing survey of **Soviet literature on air pollution** and related occupational diseases, being conducted by Ben S. Levine, is now available through the Office of Technical Services. The latest survey contains translations of 45 articles in six sections dealing with the concepts which form the basis of air pollution prevention in the U.S.S.R., the methods of approach, the scope and plan of the work, and progress evaluation. (O.T.S., U.S. Department of Commerce, Washington 25, D.C. $3.50)

Grants, Fellowships, and Awards

Grants-in-aid for research in **bacteriology and sanitary science** are available through the New York Academy of Medicine for the support of investigations, scholarships, or travel related to preventive medicine or public health. Preference will be given to applicants from the New York metropolitan area and for projects not supported by other major sources of research funds. Applications will be accepted *until 1 September*. (William C. Spring, Jr., Louis Livingston Seaman Fund, New York Academy of Medicine, 2 E. 103rd St., New York 29)

The National Science Foundation has announced that the next closing date for receipt of basic research proposals in the **life sciences** is *15 September*. Proposals received prior to that date will be reviewed at the fall meeting of the foundation's advisory panels, and disposition will be made in approximately 4 months. Proposals received after the closing date will be reviewed after the spring closing date of 15 January 1962. (Biological and Medical Sciences Division, National Science Foundation, Washington 25.)

Courses

A 2-week training course in **basic radiological health**, sponsored by the Public Health Service's Division of Radiological Health, will take place in Las Vegas, Nev., 11–22 September. The course will cover the basic nuclear physics necessary for a technical understanding of radiological problems in public health work, major sources of radiation exposure, modes of radiation injury, and units and terminology associated with this field. This course will also be given at the Radiological Health Laboratory, Rockville, Md., 23 October–3 November; at the Southeastern Radiological Health Laboratory, Montgomery, Ala., 4–15 December; and at the Robert A. Taft Sanitary Engineering Center, Cincinnati, Ohio, 9–20 October.

In addition, a 1-week course on **engineering management of nuclear emergencies**, also offered by the Public Health Service, will be held at the Southwestern Radiological Health Laboratory, Las Vegas, Nev., 25–29 September. Major attention is devoted to potential sources and types of accidents, preplanning, first stage management and follow-up, and public relations. The advanced nature of this course requires prior training in basic radiological health. (Chief, Training Program, Robert A. Taft Sanitary Engineering Center, 4676 Columbia Parkway, Cincinnati 26, Ohio)

The Latin American School of Physics, offering courses in the general field of **low-energy nuclear physics**, will be held in Buenos Aires 3 July–4 August. This school is the third in a series designed to promote the interchange of ideas among Latin American physicists by providing annually a series of intensive courses related to recent research in various fields of physics. The following courses have been scheduled: direct reactions via single-particle modes; residual forces in single-particle models; the theory of nuclear matter; angular correlations of nuclear radiations; and nuclear instrumentation. In addition to the courses, seminars on problems of high-energy, molecular, and nuclear physics will be held. (Juan Jose Giambiagi, Department of Physics, University of Buenos Aires, Buenos Aires, Argentina)

New Journals

International Review of Tropical Medicine, vol. 1, Jan. 1961. D. R. Lincicome, Ed. Academic Press, 111 5th Ave., New York 3, N.Y. $10 per volume.

Space World, vol. 1, No. 7, Apr. 1961. O. O. Binder, Ed. Spaceways, Inc., 157 W. 57th St., New York 19, N.Y. Monthly, except Feb., May, and July. $0.50 per issue.

Science News, vol. 1, No. 1, Nov. 1960. L. Bertin and H. G. Hedges, Eds. Science Publications of Canada, 25 Hollinger Rd., Toronto 16. Eight issues. $1.50 per year.

Philippine Abstracts, vol. 1, No. 1, Apr. 1960. Division of Documentation, National Institute of Science and Technology, P.O. Box 774, Manila, Philippines. Quarterly. $2 per year.

The Gerontologist, vol. 1, No. 1, Mar. 1961. O. J. Kaplan, Ed. Gerontological Society, Inc., 600 S. Kingshighway Blvd., St. Louis 10, Mo. Quarterly. $5 per year.

Cuadernos de Psicologia, vol. 1, No. 3–4, July 1959–June 1960. L. Barrios, Ed. Escuela de Psicologiá, Universidad Central de Venezuela, Caracas.

The Amateur Scientist, vol. 1, No. 4, Fall 1960. N. M. Griggs, Ed. Science League, Box 1, Boyds, Md. Irregular. $2 per year.

DSH Abstracts, vol. 1, No. 3, Apr. 1961. J. D. Schein, Ed. Deafness Speech and Hearing Publications, American Speech and Hearing Association, 1001 Connecticut Ave. NW, Washington 6. Quarterly. $8 per year.

Japanese Periodicals Index, vol. 1, No. 4, Dec. 1960. Kinokuniya Bookstore Co., Ltd., 1–826 Tsunohazu, Shinjukuku, Tokyo, Japan. $30 per year.

Scientists in the News

Hans A. Bethe, nuclear and theoretical physicist and professor of physics at Cornell University, will receive the Atomic Energy Commission's Enrico Fermi award for 1961. The $50,000 award will be presented "for contributions to nuclear and theoretical physics, to peaceful uses of atomic energy, and to the security of the United States."

The following scientists have received the 1961 College Chemistry Teacher awards of the Manufacturing Chemists' Association.

Richard M. Badger, professor of chemistry, California Institute of Technology.

Samuel P. Massie, Jr., associate program director of advanced science education programs for the National Science Foundation and chairman of the chemistry department at Fisk University.

John R. Sampey, Jr., professor of chemistry, Furman University.

Ellis L. Krause, emeritus Erwin professor of chemistry, Marietta College.

Louis A. Pappenhage, professor of chemistry, Mount Union College.

R. Nelson Smith, professor of chemistry, Pomona College.

Gerhard Herzberg, director of the division of pure physics at the National Research Council of Canada, has been elected a corresponding member of the Société Royale des Sciences de Liège.

Three members of Harvard University's Faculty of Public Health will retire this summer: **Philip Drinker**, professor and head of the department of industrial hygiene; **Hugo Muench**, professor and head of the department of biostatistics; and **Donald L. Augustine**, professor of tropical public health.

Kenneth S. Pitzer, professor and former dean of the University of California's College of Chemistry (Berkeley), has been appointed president of Rice University. Pitzer is also chairman of the Atomic Energy Commission's General Advisory Committee and head of the chemistry section of the National Academy of Sciences.

Reinout P. Kroon, former aeronautical research engineer with the Westinghouse Electric Corporation, has been appointed George Westinghouse professor of mechanical engineering at the University of Pennsylvania. Kroon has been serving as visiting professor of mechanical engineering at the university's Towne School of Civil and Mechanical Engineering since last July.

John F. Dashiell, emeritus Kenan professor of psychology at the University of North Carolina, has been appointed distinguished lecturer in psychology at Florida State University, effective in September. During the current summer session he will serve as visiting professor of psychology at the University of Rochester.

Carlton S. Proctor has been reappointed to serve as Engineers Joint Council representative to the U.S. National Commission for UNESCO.

P. Edward Byerly, formerly with the U.S. Geological Survey, Washington, D.C., has joined the staff of the La Habra Laboratory, California Research Corporation, San Francisco, as a research geophysicist.

Kenneth F. Wertman, head of the University of Arizona's department of bacteriology and medical technology, will serve as visiting professor of microbiology at the University of Costa Rica this summer, under terms of a grant from the U.S. Department of State's educational exchange program.

Franz N. D. Kurie, retired technical director of the Navy Electronics Laboratory, San Diego, Calif., recently received the Navy's Distinguished Civilian Service award for the technical programs he initiated and fostered at the laboratory.

Earl H. Wood, physiologist at the Mayo Clinic, will become the American Heart Association's tenth career investigator on 1 July.

Paul A. Scherer, former executive officer of the Carnegie Institution of Washington and treasurer of the AAAS since 1954, has been appointed special consultant to the director of the National Science Foundation.

Norman H. Jasper, former head of the Ship's Dynamic Division at the Navy's David Taylor Model Basin, Carderock, Md., has been appointed superintending scientist of the Navy Mine Defense Laboratory, Panama City, Fla.

Saul D. Larks, assistant professor of biophysics at the University of California (Los Angeles), has been appointed professor of electrical engineering at Marquette University. His new duties will include the organization of a teaching and research program in bioelectric phenomena and biomedical engineering.

C. G. Hunter, former professor of physiological hygiene at the University of Toronto School of Hygiene, Toronto, Canada, has been appointed director of Shell Research's newly established Tunstall Laboratory in Kent, England.

David R. Mitchell has been named dean of the College of Mineral Industries at Pennsylvania State University. He will continue to serve as professor and chairman of the division of mineral engineering.

Roger E. Batzel, nuclear chemist and head of the chemistry division at the University of California's Lawrence Radiation Laboratory (Livermore), has been appointed associate director for chemistry at the laboratory.

Arnold L. Demain, currently with the Merck Sharp & Dohme Research Laboratories and president of the New Jersey branch of the American Society for Microbiology, has been appointed director of the summer microbiology program for secondary school students at the University of California (Davis).

G. Böhnecke has retired after 15 years as president of the Deutsches Hydrographisches Institut, Hamburg. He will continue to serve as secretary of the Special Committee on Oceanographic Research of the International Council of Scientific Unions.

Martin Block, associate professor of physics at Duke University, has been appointed chairman of Northwestern University's department of physics.

Ernest C. Herrmann, Jr., former head of Schering Corporation's virus and tissue-culture laboratory, has been appointed virologist at the Mayo Clinic, Rochester, Minn., in the section of microbiology.

Wilbur H. Goss, assistant director for technical evaluation at Johns Hopkins University's Applied Physics Laboratory, has received the Navy Distinguished Public Service award for his work on long-range supersonic antiaircraft missiles.

Benedict E. Abreu, presently with the clinical research department of Pitman-Moore Company, Indianapolis, Ind., has been appointed professor and chairman of the department of pharmacology, University of Texas School of Medicine, Galveston.

Joseph L. Gillson, retired chief geologist for the DuPont Company, has been named William Otis Crosby lecturer in geology at Massachusetts Institute of Technology for the Academic year 1961–62.

Stanley J. Sarnoff, chief of the National Heart Institute's laboratory of cardiovascular physiology, has received the 1961 Malcolm Rogers memorial award "for excellence in the field of cardiovascular disease."

Recent Deaths

Carl Gustav Jung, 85; founder of the Jungian school of psychoanalysis; 6 June.

Frederick V. Rand, 78; editor-in-chief of *Botanical Abstracts* in the early 1930's; botany and plant pathology specialist with the U.S. Department of Agriculture from 1935 until his retirement in 1949; 6 June.

Wilber Stout, 84; retired state geologist of Ohio Division of Geological Survey; 20 May.

Robert E. Swain, 86; emeritus professor of chemistry at Stanford University and acting president of the university 1929–33; 31 May.

Erratum: The short course in medical genetics described in the announcement at the bottom of column 2, page 632, in the 3 March issue, is supported by the National Foundation rather than the National Science Foundation.

Communist Manipulation of Behavior

The Russians use confession to convict and condemn, the Chinese to rehabilitate and reform.

Lawrence E. Hinkle, Jr.

Those who study the foreboding events of our time do so under the heavy handicap of their own convictions and commitments. The mere recording of these events evokes terms that are controversial and judgments that are emotional. In 1937 when Zinoviev, Kamenev, and other founders of the Bolshevik state denounced themselves before the Moscow Tribunal as "traitors and wreckers," this was regarded as a tortured ritual of life-long revolutionaries and conspirators; but in the early 1950's, when various American and European businessmen, journalists, missionaries, and soldiers in Eastern Europe and China made confessions of extraordinary misdeeds and, in some instances, announced their support for Communism, it was taken for granted that someone had been tampering with their brains. Communists viewed this attitude as an insult and as a piece of crude propaganda. Westerners, on the other hand, embarked upon a vigorous effort to uncover the scientific perpetrators of this psychological sputnik and to understand the procedures that they had used.

The broad facts have become available and are discussed from different points of view and for different audiences in the four volumes here reviewed: **Thought Reform and the Psychology of Totalism** by Robert Lifton (Norton, New York, 1961. 510 pp. $6.95); **Coercive Persuasion** (Norton, New York, 1961. 320 pp. $6.75), written by Edgar Schein in collaboration with Inge Schneier and Curtis H. Barker; **Battle for the Mind** (Penguin Books, Baltimore, Md., ed. 2, 1961. 255 pp. $1.45) by William Sargant; and **The Manipulation of Human Behavior** (Wiley, New York, 1961. 322 pp. $7.95), a collection of seven reports edited by Albert D. Biderman and Herbert Zimmer.

The mysteries that remain are the scientific mysteries concealed behind our very limited understanding of human behavior. The techniques that have come to be called *brain washing*—the elicitation of desired confessions, the production of compliant behavior, and the apparent conversion of beliefs—are known to have evolved pragmatically, empirically, and, to a certain extent, *sui generis*, out of communist beliefs and practices, out of Russian and Chinese cultural institutions, out of police and legal procedures, and out of military and political needs. There is no convincing evidence that psychologists, psychiatrists, neurophysiologists, or scientists of any sort played any role in their planning, development, or execution. Our Russian and Chinese colleagues seem to have no better understanding of the psychological and social processes involved than we do, and perhaps they have less.

Moral Immorality

Long before the October Revolution, the Communist Party demanded the total dedication, the complete commitment, and the unswerving obedience of its members. In return, the party provided a goal based upon the future establishment of a human society free from want, cruelty, and inequality, a system of beliefs based upon mid-19th-century European science and philosophy, a clear-cut distinction between good and evil, and an apparently certain means of arriving at correct decisions on political and social questions. The party also provided a set of definitions and a system of rationalizations which made it possible to adapt the mundane realities of life to its theories and its needs. Out of the goal of reforming society arose the goal of reforming people, including prisoners. Out of the certainty that the party could distinguish right from wrong and good from evil came the certainty that it could identify the backward, the obstructionist, the reactionary, and the downright criminal and that it was the duty of the party to reform such elements in the population, or to remove them. Out of the need to explain the party's viewpoint—the people's viewpoint, the progressive viewpoint, and, in the last analysis, the only correct viewpoint—arose such concepts as the irrelevance of motives, the objective badness of the overtly good, the criminality of ultimate consequences, and the crime of merely being or thinking. If the premises of the party are correct, then its conclusions seem reasonable, as many who have been reformed will attest.

Methods, Stages, and Results

In the interest of the ultimate good, all means are equivalent, though some may be more expedient than others. In order to deal properly with those who have been designated as backward or criminal, it is first necessary to make them understand the nature of their deficiencies and, if possible, to make them sincerely repentant. It may be necessary to isolate the criminal in a place of detention or to educate him by contact with other prisoners who are at a more advanced stage in their reform. It may even be necessary to put certain pressures on him, although it is desirable not to do him bodily harm, if this can be avoided. In order to reform a man who is backward, it may be necessary to have him perform productive labor. In order to train one who would become an activist, it may be necessary to have him cut his ties with the past and to renounce people and institutions that have been most dear to him.

The preliminary stages of reform appear to the recipient as an ordeal. He may find himself arrested suddenly and inexplicably; he may find himself im-

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prisoned, in isolation or among a group of more advanced prisoners; he may be deprived of sleep and rest and comfort, and he may be subjected to a variety of physical pressures—he may even be manacled. If he is not imprisoned, he may have all of his possessions and marks of dignity stripped from him and then be shipped off to work with his hands as a laborer under trying conditions. If he is left in his home or village, he may find his whole pattern of life uprooted round him. If he should be chosen to attend a training school for cadres, his labor may be unremitting and the demands upon him incessant. At the same time, he will find himself called upon to lay bare his whole life before an examining magistrate, an interrogator, his fellow prisoners, his fellow workers, his fellow students, or the members of his village, as the case may be. His background and his actions will be criticized mercilessly. He may be derided and reviled even by his closest friends and relatives, and he may be called upon in turn to denounce them.

These experiences are, by all accounts, exceedingly trying, whatever their form and whatever their course. Frequently they produce severe disturbances of homeostasis, difficulties in mentation, delirium, and bodily illness. They almost always produce emotional disturbances, which sometimes are profound. Those being reformed become anxious, depressed, fearful, confused, and even frankly psychotic. They feel guilty, rejected, and ashamed. They are beset by profound doubts and conflicts. No small part of their reaction is the realization that they are without defenses and that many of their preconceived notions are incorrect. They become aware that, regardless of their intentions and regardless of the overt nature of their deeds, from the standpoint of the people and the socialist state, their backgrounds, the consequences of their actions, and the framework of their thinking are backward, obstructionist, and perhaps even criminal.

Russian and Chinese Purposes

However, the way to reform is always held open to them. All that is asked is that they comprehend the nature of their deficiencies, denounce their former course of action, exhibit a sincere repentance, and pursue a new course in life. If they have been judged criminal, the state will decide what penalty they must pay before they are allowed to return to full and productive membership in the socialist society. But in order to obtain this reform and rehabilitation and to escape from the situation in which he finds himself, the convicted person must follow the prescribed path, at least as far as he can. How far he must follow it depends upon the place, the person, and the circumstances. In a Russian or an eastern European prison he often need follow it only far enough to sign a protocol which has been worked out by him and the examining magistrate and which states the nature of his crimes and expresses his desire to reform. In a Chinese house of detention, he may have to undergo reform for many years, until he has given sincere evidence of having adopted the people's viewpoint as completely as he is able. If he is to become a member of the Communist Party, he must follow the dictates of the party completely and without deviation the rest of his life. If he inadvertently or unwittingly takes a position or commits an act which has consequences evil for the party, as the party judges this, he might be called upon, like the old Bolsheviks, to perform a final duty and sacrifice by revealing his crimes, denouncing them, and accepting the supreme penalty. In fact, he may have no alternative.

Psychological and Social Processes

It is largely the details of "brain washing" and the fundamental nature of the psychological and social processes involved that are still at issue. The four books here reviewed bear upon these points and provide various degrees of enlightenment with regard to them. The Lifton and Schein volumes are primarily concerned with Chinese Communist "thought reform," especially as this has been applied in civilian prisons and, to a lesser extent, as it has been applied to the Chinese population in general. The Sargant book is concerned with the entire phenomenon of "brain washing" —with its relationship to religious conversion and to Pavlovian theories and observations. The Biderman-Zimmer volume is devoted to a review of the experimental evidence bearing upon the use of physiological disturbances, reduced environmental stimulation, drugs, hypnosis, and interpersonal influence in the manipulation of human behavior. In all of these books, except the last, the ratio of explanation to information is excessively high. One would like to say to these authors (in the words of *The New Yorker*), "Just give the news, please."

The book edited by Biderman and Zimmer contains more fact and less explanation than any of the others. Its scope is more limited, and its goals are more technical; but its careful documentation of the many factors that can distort the memories, perceptions, and testimony of men should make it required reading for police officers, lawyers and jurists, as well as for military interrogators.

Lifton's book is the most readable. His case histories of Western missionaries and businessmen and of Chinese intellectuals, who spent many years undergoing thought reform, provide a vivid and thought-provoking picture of their experiences. One only wishes that Lifton did not feel called upon to explain the behavior of his informants with such certainty on the basis of psychological mechanisms that seems so uncertain to many of his colleagues. On the other hand, his later chapters, which describe the cultural perspectives of the Chinese practices, their origins and their impact on the society and the individual, are most perceptive. His comments upon what he calls "ideological totalism" and its parallels in other societies are penetrating. No student of "brain washing" should fail to read this book.

Nor should students of the subject fail to read Schein's volume, also, even though it is in many ways disappointing. The writing is more pedestrian, and in many places it is encumbered by scientific jargon. Schein and his collaborators have documented the origin and development of Chinese thought reform in a more thorough manner than anyone before them. Like Lifton, they have attempted to analyze and classify the psychological processes involved, and the effects of these upon people who have been subjected to the procedure. Unfortunately, they have decided not to provide the details of their own original observations on the subsequent behavior of 15 people of Western background who had experienced thought reform and then returned to their native societies. Their conclusions are given only in general, and they are intermingled with, and followed by, a pedantic elaboration of their own theories and a discussion of the explicit or assumed

theories of others. This may be rewarding for those who find such discussions interesting, but most readers may find it merely dull.

Sargant's book should be read for entertainment and stimulation rather than for enlightenment. It is largely concerned with Pavlov, John Wesley, voodooism, Quakerism, snake handling, and the like. There is precious little in it about what actually occurs in "brain washing." Few will go along with the author's contention that Pavlov's observations on dogs adequately explain the social phenomena just described, and fewer still would accept the suggestion that the teachings of Pavlov provided the fundamental inspiration for the whole business.

Ordering Human Society

This is not to say that there is not room—and ample room—for speculation and hypothesis. Indeed, the analogies are all too apparent. The difficulties, the threats, and the ordeals that precede confession, punishment, and reform can be likened to periods of initiation, to apprenticeship, to "plebe years," to "boot camp," to a "novitiate," in which pressures, trials, questionings, humiliations, and profound breaks with the past have been used to prepare men for their participation in religious, military, fraternal, professional, and even intellectual groups which require a high level of commitment from their members. The certainties and the rewards of a closed system of thought, accompanied by total personal dedication to high ethical goals, have been observed repeatedly in many societies over the past two millennia. The point might well be made that the conflict between this attractive way of ordering a human society and its opposite—the open system of thought, based upon observation, constantly tested against reality, allowing for great uncertainty, accepting a variety of points of view, not pretending to know the ultimate right or good, and always keeping open the possibility that any judgment is incorrect—may be the basic conflict of our time. Why so many people in so many times have chosen the first alternative, and why they have turned to it under pressure and duress is a major question at issue. Surely one can argue that physiological disturbances, Pavlovian conditioning, avoidance conditioning, symbolic death and rebirth, religious conversion, and many other social and psychological phenomena are relevant to this. But what is needed is more systematic observation, and more careful description and analysis of that which has been observed. What cultural, social, situational, historical, political, economic, and psychological factors have played what role? If beliefs, attitudes, behavior, customs, and interpersonal relations have been changed, which have been changed? To what extent? How? These questions stand before us as the greatest opportunity, and the greatest challenge, to the psychologists and sociologists of our time. It is not enough to know that the followers of Marx and Lenin have, to a certain extent, reduplicated the feats of the followers of Mohammed and of John Calvin. We should like to have a greater understanding of the nature of these phenomena in the first place.

Indians of North America. Harold E. Driver. University of Chicago Press, Chicago, Ill., 1961. 668 pp. Illus. $10.95.

Driver has written the best comprehensive book on the North American Indians of any so far published. In the broad comparative tradition of Wissler, this book is a condensation, in part, of Driver and Massey's *Comparative Studies of North American Indians*, but it is in a form much more usable for the beginning student and for the general reader. The aim stated by the author is "to offer a comprehensive comparative description and interpretation of native American cultures from the Arctic to Panama" (page v). In all, it is an accurate general text, a valuable introduction for the general reader, and a good basic reference work for the specialist; but those who seek only a general narrative or developmental work should look elsewhere. Its coverage of Middle America, Canada, and Alaska makes it more complete than any other recent text on North American Indians. The general plan was to divide Indian culture into a series of topical chapters within which the tribes are grouped by culture areas. These culture areas generally follow, with some simplification, Kroeber's natural and cultural areas.

In chapter 1, a new chapter on the origin of the Indian, Driver discusses succinctly the problems concerned with the peopling of the New World during the late Pleistocene. A welcome feature is a short criticism of the Atlantis and Mu theories of transoceanic migrations. While short, the chapter is much preferable to the rather off-hand consideration usually given to the problems of origins. In chapter 2, also new, he establishes the 14 culture areas used. Although these areas are an extreme abridgment of the complex areas established by Kroeber, they seem to be highly useful in a work of this scope.

In 12 chapters, 3 through 10 and 13 through 16, all of which are revisions and condensations of Driver and Massey's earlier comparative work, the discussion covers subsistance, horticulture, narcotics and stimulants, housing, clothing, crafts, exchange, property, family, and kin groups. Condensation has clarified and made more useful the great bulk of material found in the earlier work. Except for highly specialized professionals, these chapters seem much better than the originals. Chapters 11 and 12, on art and on music, are new and provide the same comparative treatment as the others. The 25 plates have an understandable emphasis on Meso-American art. The new chapters, 17 through 26, fill in the gaps in the cultural inventory omitted from the original. The ones on government, war, social class, clubs, life cycle, education, and language all contain a sound summary linked with a more detailed discussion by culture areas. All seem highly adequate.

This approach is varied in two chapters. In chapter 23, on religion, Driver opens with a short section defining terms and setting the stage and then turns to short sketches of religious behavior among the Aztecs, Navaho, Creek, Sanpoil, and Eskimo. A concluding section relates complexity of religious behavior to general cultural complexity.

The same plan is followed in chapter 24, on personality and culture, where the introductory material simply introduces sketches of model personalities among the Pueblo, Northeast Coast groups, Plains, Ojibwa, Iroquois, Eskimo, and modern Tepoztecans. Throughout the sketches, Driver comments on current criticisms concerning some of the older personality theories.

Chapter 26, on achievements and contributions, is an attempt to assess accomplishments in fields ranging from handicrafts or art to government or language. Borrowing by Europeans is constantly stressed in those areas where their borrowing has been significant. A greater notice might have been taken

of the way European misconceptions of Indian political structure and culture patterns strengthened current political or philosophical ideals. On the whole the final chapter lives up to the rest of the book and may well be the most interesting to the general reader.

The large pocket map of tribes is generally adequate, but many of the smaller distribution maps are difficult to use. The line drawings are often refreshingly new and show clothing styles in an active setting. Bibliography and index seem to be quite adequate. The specialist will probably find some distributions which missed his minutiae, but he will also often find them to be surprisingly thorough, accurate, and useful, even if minute documentation is lacking. I think every Americanist, general or specialized, will find this volume very useful, and I expect even students may not sell it at the end of the semester.

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Language, Culture and Personality. Essays in memory of Edward Sapir. Leslie Spier, A. Irving Hallowell, and Stanley S. Newman, Eds. Sapir Memorial Publication Fund; reissued by the University of Utah Press, Salt Lake City, 1960. 297 pp. $3.75.

The republication of the Sapir memorial volume, which first appeared in 1941, will be most welcome to this generation of linguists, anthropologists, and psychologists not simply because of its historical interest but because so many of the articles are still relevant to current issues. It is the sense of freshness and vitality that emerges from these essays, 20 years after their original publication, that is most impressive and that speaks so clearly of the genius of Edward Sapir. For this is an inspired collection, notable for the high quality of each individual contribution as well as for the way in which all 18 papers, taken together, reflect the many facets of Sapir's highly creative mind.

The contributors were either students of Sapir or those who considered themselves to be greatly influenced by him. Harry Hoijer, C. F. Voegelin, and Mary R. Haas have papers on problems of linguistic classification; Morris Swadesh, George Herzog, B. L. Whorf, and Stanley S. Newman write on linguistic behavior and thought; Clyde Kluckhohn, George L. Trager, Morris Edward Opler, M. B. Emeneau, Willard Z. Park, and Verne F. Ray present papers on the development of culture patterns; and David G. Mandelbaum, Alfred E. Hudson and Elizabeth Bacon, Wayne Dennis, Cora DuBois, and Ernest and Pearl Beaglehole deal with the relationship between culture and the individual.

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University Series in Undergraduate Mathematics. *Axiomatic Set Theory*. Patrick Suppes. xii + 256 pp. 1960. $6. *Naive Set Theory*. Paul R. Halmos. vii + 104 pp. 1960. $3.50. *Real Analysis*. Edward J. McShane and Truman Botts. ix + 272 pp. 1959. $6.60. Van Nostrand, Princeton, N.J.

Set theory has become a prerequisite for anyone interested in studying mathematics, and the theory of functions of a real variable is almost as fundamental. The concepts of set theory have pervaded the whole fabric of modern mathematics, while the Lebesque integral and other notions of real analysis have become the everyday tools of applied mathematicians and physicists. The publishers have done well to include books on these subjects in their University Series in Undergraduate Mathematics, but one must not be misled by the series title—these books are intended for the beginning graduate student and at best are suitable for the mature senior undergraduate.

Naive Set Theory and *Axiomatic Set Theory* cover essentially the same material from different viewpoints and with different aims. Halmos wants to provide the aspiring mathematician with the background he needs to work in his chosen discipline. Thus Halmos gives an informal exposition of set theory and introduces any axioms needed to establish those properties of sets required by mathematicians. This informal treatment suffers from the absence of any emphasis on the important facts of set-theoretic life. The reader, carried away by the author's conversational style, thinks everything is clear and simple, only to be brought face to face with difficult proofs toward the end of the book. Suppes is concerned with the subject of set theory as a discipline with intrinsic interest. Accordingly, he follows a rigorous procedure of axioms, definitions, and theorems, although he presents many proofs informally. The theory developed is based on the Zermelo-Frankel system which avoids the set-theoretic paradoxes by certain devices that run counter to modern mathematical developments which usually follow the von Neumann formulation. However, since the author is principally concerned with a consistent theory, he has justifiably chosen the simplest direction.

Real Analysis comprises within its covers a tremendous amount of material from the domain of real function theory. This is accomplished by keeping the number of examples and counterexamples very low. While one could forego examples with some reservations, the lack of counterexamples is a severe loss. In subjects where the theorems are very complicated and contain many hypotheses, it is very important to show the part played by every hypothesis. This can best be done by counterexamples which fulfill all the hypotheses but one and for which the conclusion does not hold. However a good instructor who chooses this otherwise fine book for his text can compensate for this omission.

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Traité de Zoologie. Anatomie, Systématique, Biologie. vol. 5, pt. 1, Annélides, Myzostomides, Sipunculiens, Echiuriens, Priapuliens, Endoproctes, Phoronidiens; pt. 2, Bryozoaires, Brachiopodes, Chétognathes, Pogonophores, Mollusques (Généralités, Aplacophores, Polyplacophores, Monoplacophores, Bivalves). Pierre-P. Grasse, Ed. Masson, Paris. pt. 1, 1116 pp. + plates, 1959; pt. 2, 1186 pp. + plates, 1960. Paper, NF. 180; cloth, NF. 190 each.

This magnificently printed series continues on its way; when completed it should be a monument to French zoology. The first fascicule of volume 5 concerns worms and some creatures which are "vermidien" in the sense of that word as it was used by eminent French zoologists of the last century. Unfortunately the polychaetes are not treated too well; more space is given to

the oligochaetes (by about 25 pages). There is a separate treatment of annelid embryology which gathers up some of the loose ends. Other loose ends are appended in the substantial addendum. The arrangement of this fascicule leaves one with the impression that something —perhaps the death of Fauvel—interfered with its orderly preparation.

The second fascicule is very up to date, with translations of Ivanov on Pogonophora and of Lamche and Wingstrand on Monoplacophora. The section on Chaetognaths is rather casual and considerably shorter than Hyman's treatment, but the section on the Ectoprocta is much longer. Other groups discussed in both works—such as Phoronida, Brachiopoda, and Sipunculida —receive about equal treatment. It is unfortunate that the mollusks have to be split between two of these very expensive "fascicules"; however, it appears that the total space to be given to mollusks would make an unwieldy single volume.

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The Encyclopedia of Microscopy. George L. Clark, Ed. Reinhold, New York; Chapman and Hall, London, 1961. xiii + 693 pp. Illus. $25.

Microscopy, for many years pioneered by the biologist, is now everybody's helper. Small and large books, specialty monographs, sections in multivolume sets, and now a specialty encyclopedia are available. About a century ago there was the Griffith and Henfry *Microscopic Dictionary*. The present volume summarizes the material by types of microscopy and describes microscopes then unknown. Unfortunately costs now preclude the colored plates that were included in the older books.

The contributions (with number of pages) of 76 authors are organized into sections as follows: autoradiography (13), chemical (59), electron (259), fluorescence (2), flying spot (5), forensic (5), general (57—fibers, industrial, microscopists and management, microtomy, plastics, pulp and paper), industrial hygiene (11), infrared (1+), interference (23), light "(optical)" microscopy (34), metallography (0, referenced to electron and general), micrometron automatic (1), microradiography (0, referenced to x-ray), optical mineralogy (6), phase (4), polarizing (6), refractometry and interferometry (40), resinography (14), stereoscopic (5, includes Gregory's solid image), television (2), ultramicroscopy (0, see light microscope), ultrasonic absorption (4), ultraviolet (13), and x-ray (33).

The volume is not tightly edited, and the style and content of the articles are uneven. Some duplication occurs—for example, interference and x-ray. Many of the contributions are comprehensive, a few—for example, fluorescence, infrared, and television—are too brief to be very useful. Forensic microscopy is limited to bullet comparisons. The section on the petrographic microscope ignores recent applications to opaque minerals. Ultrasonic microscopy misses instrumentation used in ophthalmology. Many articles have good historical sections—for example, that on electron optics—but in general the historical article does not reveal the development of the instrument or of microscopy and scarcely mentions British and American contributions. On page 462 we read that fluorescence microscopy "was first mentioned in publications in 1929 . . . "—yet on page 333 references to 1911 papers are cited! At least two authors use the Abbe formula for resolution without mention that it is now known to be overconservative by some 20 percent.

While the many cross references are helpful, the lack of an index restricts the utility of the book. On looking through the volume one finds useful information and tests which would otherwise be hard to locate. A section on recording observations (drawing, photomicrography, reconstructions, and the like) would be a helpful addition.

The book is well illustrated, especially the electron microscope section which has pictures of biological cell ultrastructure. Most of the contributions provide adequate references for further reading. The increasing use of the microscope in industry is demonstrated. The encyclopedia tells about some 26 kinds of microscopes and microscopy rather than how to use the instruments. It is a reference book that should call attention to the possibilities of microscopy and lead to greater opportunities for professional microscopists.

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New Books

Social Sciences

The Antitrust Laws of the United States of America. A study of competition enforced by law. A. D. Neale. Cambridge Univ. Press, New York, 1960. 535 pp. $7.50.

Business Cycle Indicators. vol. 1, *Contributions to the Analysis of Current Business Conditions.* Geoffrey H. Moore, Ed. Princeton Univ. Press, Princeton N.J., 1961. 792 pp. $12.50.

Challenge of Psychical Research. A primer of parapsychology. Gardner Murphy. Harper, New York, 1961. 315 pp. Illus. $6.

The Changing Nature of Man. Introduction to a historical psychology (metabletica). J. H. Van den Berg. Norton, New York, 1961. 252 pp. $4.50.

Chronic Schizophrenia. Lawrence Appleby, Jordan M. Scher, and John Cumming, Eds. Free Press, Glencoe, Ill., 1960. 383 pp. Illus. $6.

Congo, Background of Conflict. Alan P. Merriam. Northwestern Univ. Press, Evanston, Ill., 1961. 382 pp. $6.

Creativity and the Individual. Summaries of selected literature in psychology and psychiatry. Morris I. Stein and Shirley J. Heinze. Published for the Graduate School of Business, Univ. of Chicago, by Free Press, Glencoe, Ill., 1960. 438 pp. $10.

Curiosity. Herman Nunberg. International Universities Press, New York, 1961. 88 pp. An expanded version of the Freud Anniversary Lecture given at the New York Academy of Medicine in May 1960.

The Effects of Leadership. Hanan C. Selvin. Free Press, Glencoe, Ill., 1960. 284 pp. $5.

Ethnographic Bibliography of North America. George Peter Murdock. Human Relations Area Files, New Haven, Conn., ed. 3, 1960 (order from Taplinger, New York). 416 pp. Illus. $6.75.

Experimental Psychology. Frank J. McGuigan. Prentice-Hall, Englewood Cliffs, N.J., 1960. 320 pp. $6.

From Adolescent to Adult. Percival M. Symonds, with Arthur R. Jensen. Columbia Univ. Press, New York, 1961. 427 pp. $8.75. A follow-up study to *Adolescent Fantasy* (1949).

More Resources for Education. John Dewey Society annual lecture, Chicago, February 1960. Seymour E. Harris. Harper, New York, 1960. 96 pp. $2.95.

Philosophies of Education. Philip H. Phenix. Wiley, New York, 1961. 137 pp. Paper, $1.50. Thirteen articles based on a series of educational television programs produced by KTCA-TV for the National Education Television and Radio Center.

The Principles of Scientific Research. Paul Freedman. Pergamon, New York, ed. 2, 1960. 245 pp. Illus. $4.50.

Rural China. Imperial control in the 19th century. Kung-Chuan Hsiao. Univ. of Washington Press, Seattle, 1960. 797 pp. $9.75. A study of the rationale, methods, and effects of the system of control over rural China as exercised by the Ch'ing government during the 19th century.

Reports

Estimation of Total Body Fat from Potassium-40 Content

Abstract. A brief review is given of the development of methods for the determination of total body potassium by whole-body scintillation counting and of studies to establish the utility of body potassium as a measure of gross body composition including fat.

In a paper of the above title (*1*), it is stated that "a new approach to the estimation of fat content in living man" is presented, "namely, the use of whole-body potassium content as an index of lean body mass." The potassium is determined by measurement of the gamma-ray emission of potassium-40. Far from being new, this method can be traced back to 1951 and has been under study by the Biomedical Research Group of the Los Alamos Scientific Laboratory since 1953. (The following literature review covers only the determination of body composition by counting natural gamma radiation. The very important complementary studies of exchangeable potassium are not included.)

Historically, the first determinations of body potassium by means of its natural radioactivity were reported by Sievert (*2*) and by Burch and Spiers (*3*). Although this early work was directed primarily toward the determination of small additional amounts of other gamma-emitting nuclides (principally radium and thorium), Sievert (*2*, *4*) established the change of the potassium content of the body with age and indicated the explanation in terms of body composition.

The measurement of potassium by whole-body scintillation counting of potassium-40 was first reported in 1953 by Reines *et al.* (*5*), who used a 4π liquid-scintillation counter, and in 1956 by Miller and Marinelli (*6*), who used a sodium-iodide crystal. Similar measurements were published later by McNeill and Green (*7*). The correlation between total body potassium and body water was demonstrated by Woodward *et al.* (*8*) in a paper in which the relation between potassium, lean body weight, and fat was discussed. In particular, the use of simultaneous water and potassium determinations to detect deviations from the normal was proposed. Such a procedure will undoubtedly be necessary for high precision work, since the body is not a simple two-component system.

Clarification of the details of the relationship between potassium and body composition has been a continuing major research program of the Biomedical Research Group since our 1953 measurements. In 1957, the correlation between potassium and lean body mass (as deduced from body water) was reported to be reliable to a standard deviation of 6 percent (*9*). As a more specific proof of the correlation, additional experiments were performed on other species so that verification by dissection and chemical analysis would be possible. Published reports include papers by Kulwich *et al.* on hams (*10*), by Zobrisky *et al.* on live pigs (*11*), and by Anderson on comminuted products (*12*).

Work on human subjects was extended with the report in 1959 (*13*) of measurements on 1590 persons with particular reference to variation of potassium with age and sex and the explanation in terms of fat and lean body weights. Detailed quantitative data were published by Allen *et al.* (*14*) comparing lean body weights calculated from potassium, water, and body density (determined by underwater weighing). The agreement between the various methods was shown to be excellent (standard deviation of 5 percent). The poor correlation reported by Forbes *et al.* (*1*) between potassium and skinfold thickness and height-to-weight ratios suggests that the latter two parameters provide poor estimates of body fat. In view of the long published history of research directed toward the determination of fat and of lean body mass from potassium as measured by whole-body gamma counting, it hardly seems that their statement of a "new approach" is justified.

In addition to this brief review of the historical development of the whole-body counting method, we wish to comment on the assumption by Forbes *et al.* (*1*) of a linear relation between water, potassium, and lean body weight. Our 1956 paper (*8*) used the 72 percent ratio of water to lean body weight, with the result that K (meq)/LBW = 63 meq/kg, compared with Forbes's value of 68. In a latter paper (*9*), we have shown that a better fit between water and potassium is obtained if allowance is made for the fact that fat tissue contains about 20 percent water (see *15*). [Allowing 31 percent water in fat tissue (*16*) apparently results in an over-correction.] The result, on this basis, was that K/LBW = 73 meq/kg, 8 percent higher than Forbes's ratio.

A basic difficulty in this sort of analysis is the well-known problem of defining "lean body mass" with precision. For this reason, we have preferred in our later work (*14*) to base the analysis of body composition on chemically defined components as suggested by Allen (*17*).

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3 February 1961

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to contributors" [*Science* **125**, 16 (1957)].

The report by Anderson and Langham begins with an excellent historical survey of whole-body counter methodology. No one can deny the importance of the contribution which they and their co-workers have made in this field. As a result of their studies there are now available more data on the potassium content of man than for any other body constituent. But such a detailed exposé was clearly beyond the scope of our brief report in *Science* (*1*), nor is it germane to the substance of that report.

We considered the *technique* of potassium measurement to have been clearly established. Our intent was to develop an *application* of that technique, and this is what we did.

Apparently Anderson and Langham are not content with the references which we made (references 5, 6, 10) to their work (*2*). We have searched the writings of the Los Alamos group without finding more than oblique and vague references to fat determination in man by this technique. Mention is made (which we quoted) of lean body mass, and much emphasis is placed on the concept of bone mineral-free, water-free, and fat-free solids. It is surprising indeed that they could have been working on a method for fat determination since 1953 without once having published a value for actual fat content for any of their subjects. There was nothing to hinder them from doing so, and one can only conclude that they were not particularly interested in this aspect of body composition. Indeed, we ourselves have made such calculations from their data, which they have so generously made available to us, often in advance of publication.

In all fairness to history, it should be stated that the concept of the constancy of adult tissue composition when calculated on a fat-free basis long antedated the establishment of the Los Alamos group (*3*). In recent years a number of workers have applied this general principle to fat determination in living man by the total body water and densitometric techniques. Our intent was merely to suggest an extension of this principle to include potassium.

Our interest in this problem dates from some analyses of human carcasses made in 1954 and published in 1956 (*4*). Upon completion of the Rochester whole-body counter, measurements were made in 1959 and first reported at the May 1960 meetings of the American Pediatric Society. The calculations are based, not upon indirect estimates of fat content, but upon actual chemical analyses, and despite the protestations of Anderson and Langham, our work is the first to be published in which emphasis is placed on the estimation of fat content by means of the potassium technique in living man.

We are quite aware of the importance of the issue raised in the next to the last paragraph of their report. The question of the composition of the "lean body mass" in living man is one that can be approached only by indirect and inferential means, and inevitably involves assumptions based on the many animal and the all too few human analyses which have been done. As Anderson and Langham have indicated [and the recent data of Talso *et al.* (*5*) would bear them out], the potassium content of lean body mass calculated from simultaneous water measurement differs by a few percent from that derived from chemical analysis of carcasses. It is impossible to decide whether this discrepancy is due to a systematic error in the total body water method, or to the possibility that the analyzed specimens were not representative of the normal state.

At any rate, we were encouraged to proceed with our work by the report from the Los Alamos group—and we quote from our report (*1*)—of "a good correspondence between K^{40} content and lean body mass as determined by tritium dilution in man."

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References and Notes

1. G. B. Forbes, J. Gallup, J. B. Hursh, *Science* **133**, 101 (1961).
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23 February 1961

Sound Production in Scorpions

The fact that some species of scorpions produce sound has been known for a long time. It was suggested by Landois and Wood-Mason in 1877 (from Werner, *1*) and recorded for the first time by Pocock (from Alexander, *2*). Several observations on the behavior of scorpions, morphological studies, and attempts to draw sound from preserved specimens led to the suggestion that various body parts might be involved in sound production by scorpions (summarized by Werner, *1*).

The subject was investigated in detail only recently, by Alexander (*2*), in *Opisthophthalamus latimanus* Koch., *O. nitidiceps* Poc. (Scorpionidae), and *Parabuthus planicauda* Poc. (Buthidae). The two species of the genus *Opisthophthalamus* were found to produce a hissing sound by rubbing the chelicerae against the ventral side of the carapace; whereas *Parabuthus planicauda* produced a shrill grating sound by scraping the tip of the tail across the first caudal tergite and the last mesosomal one. Vachon *et al.* (*3*) registered in 1958 the specters and oscillograms of the sound produced by *Opisthophthalamus latimanus*.

So far all mechanisms of sound production in scorpions, either suggested or actually investigated, have been based on stridulation—that is, friction between two body parts which may, or may not, bear special adaptations for sound production.

During studies on *Scorpio maurus* L. (Scorpionidae), we found that this scorpion produces sound by a mechanism which is basically different from the mechanisms mentioned above.

At least two subspecies of *S. maurus* occur in Israel: *S. maurus fuscus* H. et E., which varies in color from light to dark brown, and *S. maurus palmatus* H. et E., which is yellow (*4*). Both these subspecies produce sound by rapidly striking the posterior half of the mesosoma against the ground. While they are doing so, their pedipalps are flexed, the tail is brought over the mesosoma until it is almost touching it, and the body vibrates rapidly in a forward-upward, backward-downward direction. No other part of the body, such as the chelicerae, pectines, or aculeus, is involved in this reaction. In *S. maurus palmatus* each strike is accompanied by a few very rapid vibrations of the body which are performed without hitting the ground, so that the effect produced is that of a series of very rapid, successive, clearly audible thuds. *Scorpio maurus fuscus* was observed to hit the ground at a higher frequency; the motion is so rapid that it is difficult to ascertain whether each jerk actually hits the ground or not. In this case the successive thuds merge into a rattle.

Unlike the hissing of *Opisthophthalamus*, which may precede an encounter between two specimens, but will not occur after the two have come into actual contact, as stated by Alexander (*2*), *Scorpio maurus* may produce sound after a fight with another specimen. This response was witnessed also when a scorpion was following a locust which it failed to catch.

Among *S. maurus fuscus* a smaller form (possibly another subspecies) occurs, the adults of which do not surpass 5 cm in length. In this case similar vibrations were also observed in the

male (once after the escape of a female with which the male was trying to mate), but no audible sound could be detected.

It is extremely difficult to induce *S. maurus* to produce sound, but we succeeded in eliciting this response several times by the use of irritating stimuli. Once a scorpion has begun to respond in this manner, after either a natural or an artificial stimulus, it will often repeat the response a few times, running around and stopping every now and then in order to produce the sound. So far only males have been observed to perform this type of reaction, but the possibility that females too may be able to do so has not been excluded.

Stridulation is a very common method for sound production among insects (*5*) and crustaceans (crabs) (*6*), but cases of sounds produced by rapidly tapping any part of the body on the substrate (or "drumming," as the mechanism is often termed) also occur in both these classes. Thus Imms (*5*) lists cases among Anobiidae, pupae of Hesperiidae and Lycaenidae, termite soldiers, and Psocotera. Drumming in *Pimelia striata* is mentioned by Darwin (*7*) and in some Acrididae by McE-Kevan (*8*) and Pierce (*9*). Crane (*10*) describes drumming in some crabs of the genus *Uca* (Brachyura). However, to the best of our knowledge, drumming in scorpions has never been recorded before.

The body vibrations of *Scorpio maurus* during sound production recall in their tetanic nature the juddering of males of *Opisthophthalamus latimanus* (Scorpionidae) (*11*) and *N. hierochonticus* (Diplocentridae) (*12*) during courtship, except that in the latter case the pedipalps are outstretched and the motion is performed horizontally, so that the body does not hit the ground. The juddering of scorpions is not limited to sexual behavior and may occasionally be performed in the presence of other males, specimens of another species, and sometimes even in the presence of prey. However, emission of sound in *Scorpio maurus* does not constitute a part of the normal courtship behavior.

Scorpio maurus exhibits various types of threatening behavior and also repeated forward and backward jerks of the body with outstretched pedipalps and arched tail. This reaction is very common in both sexes. (An identical, or at least a very similar, reaction is almost invariably performed by the male at the beginning of courtship.) There may be some relation between this threatening behavior and sound production, since scorpions were observed several times to change such a threatening reaction into the sound-producing one. The body jerks were quickened, the pedipalps were gradually flexed, and the tail was lowered over the body; the sound was at first very feeble, but soon acquired full volume.

It is not yet known whether the difference observed in the frequency of the sound by the two subspecies *S. maurus palmatus* and *S. maurus fuscus* is innate.

No response of one scorpion to the sound produced by another was noted. Alexander (*2*), who found no such response in the cases she investigated (stridulatory type), suggested after making a few experiments that the sound may have a defensive value against predators. The presence of a sense of hearing in scorpions is still doubtful, but considering that their sensitivity to vibrations of the substrate is apparent (*13*), the possibility that, in the case of *S. maurus*, sound may have also an interspecific role is not to be dismissed without further careful investigation.

Variations in the loudness of the sound, due to changes in intensity of reaction of one and the same specimen, were mentioned above. We realize, however, that the loudness of the sound may also depend, at least partially, on the hardness of the substrate against which the body hits, although slightly pressed soil suffices for the production of a clearly audible sound. It should also be noted that scorpions do not usually inhabit terrains of soft loose sand (*14*).

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Theory of Oxygen Transport through Hemoglobin Solutions

Abstract. A combination of equations describing diffusion, gas solubility, and mass conservation yields a general relation for oxygen transport in hemoglobin solutions. Total oxygen flux is shown to be the sum of the flux in the plasma and the flux due to transport by hemoglobin. The total flux depends not only on oxygen concentration gradient but also on absolute oxygen concentration.

Because of the recent interest in the transport of oxygen in hemoglobin solutions (*1*, *2*), it seems desirable to attempt to understand this transport phenomenon in terms of simple physical-chemical principles.

The theory that one generally applies to transport phenomena is embodied in the statement of Fick's law (*3*). This law states that the mass transport per unit time per unit area, $\partial m/\partial t$, is directly proportional to the concentration gradient, $\partial c/\partial x$, where the constant of proportionality, D, is called the diffusion coefficient. That is

$$\frac{\partial m}{\partial t} = - D \frac{\partial c}{\partial x} \qquad (1)$$

For the problem at hand we must consider the diffusion of three species, namely, oxygen dissolved in plasma or water, oxygenated hemoglobin, and hemoglobin. We choose a geometry of plane parallel boundaries so that the problem is one dimensional. Let A be the concentration of dissolved oxygen, B the concentration of hemoglobin, and E the concentration of oxygenated hemoglobin.

We may now write three simultaneous diffusion equations:

$$J_A = \frac{\partial m_A}{\partial t} = - D_A \frac{\partial A}{\partial x}$$

$$J_B = \frac{\partial m_B}{\partial t} = - D_B \frac{\partial B}{\partial x} \qquad (2)$$

$$J_E = \frac{\partial m_E}{\partial t} = - D_E \frac{\partial E}{\partial x}$$

These equations are not independent and must satisfy certain boundary conditions. Since hemoglobin and oxygenated hemoglobin cannot spill over the boundary of the diffusion cell, we must require that

$$J_B + J_E = 0 = - D_B \frac{\partial B}{\partial x} - D_E \frac{\partial E}{\partial x} \qquad (3)$$

A further constraint on the system is imposed by conserving the total amount of A and B and remembering that $A + B \rightleftharpoons E$. The material balance for oxygen (A) implies that

$$D_A \frac{d^2A}{dx^2} + D_E \frac{d^2E}{dx^2} = 0 \qquad (4)$$

while the material balance of hemoglobin (B) similarly requires that

$$D_B \frac{d^2B}{dx^2} + D_E \frac{d^2E}{dx^2} = 0 \qquad (5)$$

For the case assumed here, namely that D_A, D_B, and D_E are independent of concentration, the general solutions of Eqs. 4 and 5 may be written as

$$D_A A + D_E E = a_1 x + a_2 \qquad (6)$$

and

$$D_B B + D_E E = a_3 x + a_4 \qquad (7)$$

where a_1, a_2, a_3, and a_4 are constants to be determined by the boundary and subsidiary conditions. At the two boundaries, $x = 0$ and $x = x_f$, we require the following conditions:

$$\text{at } x = 0, \qquad A = A_i, \qquad (8)$$

the initial value of A

$$\text{at } x = x_f, \qquad A = A_f, \qquad (9)$$

the final value of A, and,

$$B = B_f,$$

the final value of B.

There is only one other condition needed to specify uniquely a solution to Eq. 2 and that is the relation between A and E. When one has a reaction of the form $A + B \rightleftharpoons E$, he is often tempted to describe the equilibrium situation as follows:

$$E/AB = K \qquad (10)$$

where K is the equilibrium constant. This is not a good description in the case of the oxygen-hemoglobin equilibrium. The oxygen-hemoglobin dissociation curve, Fig. 1 (*4*), cannot be fit by an equilibrium expression of the form of Eq. 10. We therefore choose to write E as a function of A and use the curve of Fig. 1 to define the function. Therefore

$$E = f(A) \qquad (11)$$

Having enumerated all the assumptions, constraints, and boundary conditions, we now wish to calculate the total transport of oxygen whether it be as dissolved oxygen or as oxygenated hemoglobin. This means that we wish to find the sum of J_A and J_E.

$$J_A + J_E = D_A \frac{\partial A}{\partial x} - D_E \frac{\partial E}{\partial x} \qquad (12)$$

By differentiating Eq. 6 with respect to x we obtain

$$J_A + J_E = -a_1;$$

the value a_2 may be evaluated from the boundary conditions if $x = 0$ is applied to Eq. 6

$$a_2 = D_A A_i + D_E f(A_i) \qquad (13)$$

Substitutions from Eq. 13 for a_2 and solving for $-a_1$ gives us

$$J_{\text{total}} = -a_1 = \frac{D_A(A_i - A_f) + D_E[f(A_i) - f(A_f)]}{x_f} \qquad (15)$$

The significance of this equation is that the transport of oxygen arises from two sources: direct diffusion of the dissolved gas, and transport as a complex with hemoglobin.

We can write Eq. 15 in terms of the initial and final partial pressures of oxygen by use of Henry's law which relates concentration to pressure as follows:

$$A = kP \qquad (16)$$

Perhaps it should be stressed at this point that, although the concentration of oxygen dissolved in plasma or water is related to the pressure by Henry's law, this is not true for the oxygen complexed by hemoglobin. The relation between the oxygen partial pressure and the fraction of hemoglobin complexed is given by Fig. 1.

Combining Eqs. 15 and 16 gives

$$J = \frac{D_A k\ (P_i - P_f)}{x_f} + \frac{D_E[f(kP_i) - f(kP_f)]}{x_f} \qquad (17)$$

The first term in Eq. 17 is due to the direct diffusion of the dissolved gas, and as long as the pressure difference, $P_i - P_f$, remains constant it is independent of the value of P_i or P_f. The second term, however, is not only dependent on the pressure difference, but is also sensitive to the absolute value of P_i and P_f as may be clearly understood by an examination of Fig. 1. If P_i and P_f are each so large that the hemoglobin is saturated with oxygen at both boundaries of the transport path, then the second term in Eq. 17 will be negligible compared to the first.

In the light of these results let us consider the results of Hemmingsen and Scholander (*2*) on the transport of oxygen in hemoglobin solutions. One of their experiments was to keep the pressure difference, $\triangle P$, across a diffusion cell constant but to vary P_i and P_f. That is to say they maintained the constraint

$$P_i = P_f + \triangle P$$

They observed a maximum transport for $P_f = 0$ followed by a sharp drop as

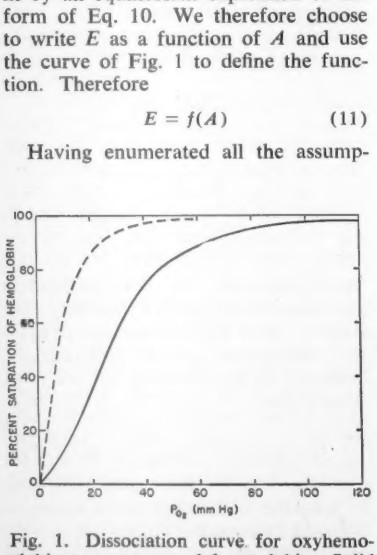

Fig. 1. Dissociation curve for oxyhemoglobin to oxygen and hemoglobin. Solid line is for hemoglobin at 38°C and *p*H 7.4 (after Comroe *et al.*, *4*). Dashed line is for Hemmingsen and Scholander (*2*) hemoglobin solution at 23°C and *p*H 7.3.

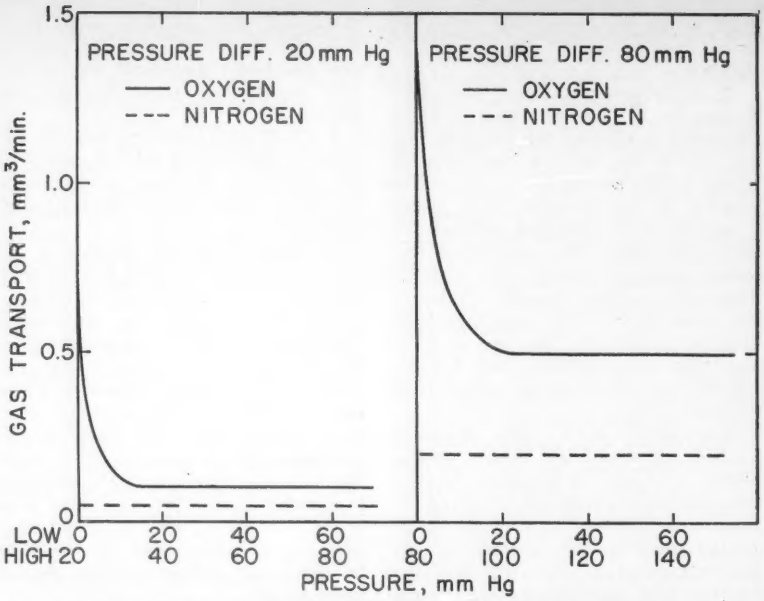

Fig. 2. Flux of oxygen and nitrogen through hemoglobin solution at constant pressure gradients but varying absolute pressure (after Hemmingsen and Scholander, *2*).

P_f was increased and finally a constant transport for large P_f. Their results are shown in Fig. 2. Their experimentally determined oxygen-hemoglobin dissociation curve is given as the dashed curve in Fig. 1. We readily see that these results are in qualitative agreement with Eq. 17. When P_f is zero $f(kP_i) - f(kP_f)$ takes its maximum value, and as P_f is increased to the point where the hemoglobin is largely saturated, $f(kP_i) - f(kP_f)$ decreases, monotonically approaching zero. As the second term in Eq. 17 becomes negligible, all the oxygen transport is due to dissolved gas and is therefore independent of P_f.

We feel that these considerations make it clear, by and large, how hemoglobin is effective in oxygen transport. That is, if the hemoglobin is under conditions such that it is saturated with oxygen at both ends of the transport path, it cannot be effective at all.

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19 December 1960

Effects of Plant Nutrients on Uptake of Radiostrontium by Thatcher Wheat

Abstract. The effects of various dosages of ammonium dihydrogen phosphate, monocalcium phosphate, calcium chloride, and potassium chloride on the uptake of radiostrontium by Thatcher wheat grown in Saskatchewan Oxbow loam soil containing strontium-85 were studied. Monocalcium phosphate at a dose level of about 600 lb/acre of soil effected a statistically significant reduction of strontium-85 uptake in each of the four plant fractions of grain, chaff, stem, and leaf. At the very reasonable dosage of about 60 lb/acre, monocalcium phosphate gave a statistically significant reduction in strontium-85 uptake in the grain and chaff.

In 1958, Libby (*1*) reported that addition of solutions of potassium salts to a Washington garden soil containing Sr^{90} had a beneficial effect in reducing the uptake of the Sr^{90} by radish plants. His conclusions were criticized (*2*) because the soil was not fully characterized and the experiments lacked sufficient replication. There is no dispute, however, on the need for more research on methods of soil treatment which may lead to a reduction in plant uptake of Sr^{90} (*3*). Recently, Fowler and Christenson (*4*) described a study on the effect of soil nutrients on plant uptake of fallout, and reported that increasing the concentration of soil calcium has a depressing effect on the concentration of Sr^{90} per milligram of calcium in lettuce, grass, and alfalfa. In an investigation with bean plants on the relative availability of a number of Sr^{90} compounds in soil, Uhler and Hungate (*5*) noted the low availability of strontium phosphate, especially in calcareous soils, thus suggesting the possibility of the use of phosphate fertilizers as a means of reducing Sr^{90} uptake. However, it was stated by these authors (*5*) that, although their results indicated that uptake of Sr^{90} might potentially be reduced through precipitation reactions, initial experiments with phosphate fertilizers did not permit much optimism on this point.

As an extension of our earlier work on the uptake and distribution of Sr^{90} in Thatcher wheat (*6*), the effects of a number of plant nutrients on the uptake of Sr^{85} by Thatcher wheat were investigated (*7*). The plants were grown, three per pot, in the greenhouse. Each pot contained 400 g of Saskatchewan Oxbow loam soil which had a *p*H of 7.2, and 19.2, 6.5, 1.9, and 0.1 meq of exchangeable Ca, Mg, K, and Na, respectively, per 100 g of soil. Before the seeds were sown, each pot of soil was first mixed thoroughly with 50 ml of Sr^{85} (*8*) solution (1 μc/ml) and then with 50 ml of nutrient solution or with 50 ml of distilled water as a control. The plant nutrients, used at various dose levels as given in Table 1, were ammonium dihydrogen phosphate, monocalcium phosphate, calcium chloride, and potassium chloride. Each treatment was repeated in six pots. The seeds were germinated, and the plants were grown to maturity. The soil moisture was kept at field capacity by daily watering. After harvest, individual plants were separated into grain, chaff, stem, and leaf and analyzed for radioactivity. Since three plants were grown per pot and each treatment was repeated in six pots, analyses from 18 replications were obtained.

Table 1. Uptake of strontium-85 by Thatcher wheat grown in Saskatchewan Oxbow loam soil. Each value for uptake is the mean of 18 replications.

Nutrient treatment	Dosage (meq/100 g of soil)	Uptake (%) per gram of dried tissue			
		Grain	Chaff	Stem	Leaf
None (control)		0.0106	0.026	0.071	0.376
$NH_4H_2PO_4$	0.08 PO_4*	.0116	.027	.071	.378
	0.16 PO_4	.0100	.026	.065	.323†
	0.80 PO_4	.0101	.031†	.066	.338
$Ca(H_2PO_4)_2$	0.08 PO_4, 0.027 Ca*	.0088†	.021†	.070	.399
	0.016 PO_4, 0.054 Ca	.0089†	.026	.073	.390
	0.80 PO_4, 0.27 Ca	.0078†	.023†	.061‡	.324†
$CaCl_2$	0.027 Ca	.0090†	.025	.068	.349
	0.27 Ca	.0097	.025	.062	.343
KCl	1.9 K§	.0092	.021†	.059	.351
	3.8 K	.0103	.023†	.065	.386
L.S.D.‖		.0016	.002	.009	.047

* This dosage is equal to a treatment of about 60 lb. of nutrient per acre of soil, assuming the weight of 1 acre of soil to be 2×10^6 lb. † Significant at the 1 percent level. ‡ Significant at the 5 percent level. § This dosage is equal to the amount of exchangeable potassium already present in the soil. ‖ Least significant difference.

Inspection of Table 1 shows that ammonium dihydrogen phosphate, calcium chloride, and potassium chloride, when applied at dose levels used in these experiments, did not give any consistent pattern in reducing Sr^{85} uptake. It may be pointed out that as a means of expressing the data in a uniform way, the accumulations of strontium are given in Table 1 as the percentage uptake per gram of dried tissues instead of total uptake in various parts of the plant. With only one exception, the various nutrient treatments did not influence average yields by more than a few percent as judged by the weights of the grain. The exception was the treatment with the highest dose of ammonium dihydrogen phosphate, where an increase in average yield of about 30 percent was noted. This may bring in a carbohydrate dilution factor when the uptake of strontium per gram of dried tissue was calculated. However, statistical analysis of the data expressed as the percentage of total uptake also showed that treatments with ammonium dihydrogen phosphate did not cause any statistically significant difference from total uptakes in the control. Of the ten nutrient treatments given in Table 1,

only monocalcium phosphate, a component of "superphosphate" fertilizer, applied at the highest dose level of about 600 lb/acre, effected a statistically significant reduction of Sr^{85} uptake in all four fractions of the wheat plant. Even at the very reasonable dosage of about 60 lb/acre, monocalcium phosphate gave a statistically significant reduction in Sr^{85} content in the grain and chaff. These results appear to indicate that a combined utilization of both the calcium effect (*4*) and the phosphate effect (*5*) may have some promise as a means of soil treatment to reduce plant uptake of radiostrontium.

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9. Sincere thanks are extended to Dr. F. Sosulski for doing the statistical analysis.

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Vitamin B_{12} Requirement of a Marine Blue-Green Alga

Abstract. A species of *Synechocystis* isolated from a marine mud has an absolute requirement for vitamin B_{12}. Analogues of B_{12}, including cobinamide and α-(adenyl) cobamide cyanide, satisfactorily substitute for vitamin B_{12}. Methionine alone or in combination with other compounds supports only a low level of growth.

Recent discoveries that many phytoplankton organisms require vitamins (*1*), notably B_{12}, point to vitamin cycles as significant determinants of the productivity of the sea. Blue-green algae are of widespread occurrence in marine environments and are undoubtedly important contributors to the productivity of these areas. Despite this, little is known of their growth and nutrition. A blue-green alga belonging to the genus *Synechocystis* (termed 17a in our laboratory), isolated from a mud sample from Long Island Sound, has been found to have an absolute requirement for vitamin B_{12}. Its B_{12} requirement and possible sparing action of certain compounds were investigated in detail.

The ASP-2 medium of Provasoli (*2*), modified by increasing phosphate 10-fold and nitrate 20-fold, initial *p*H 8.2, has been found suitable for the enrichment and isolation of many marine blue-green algae, and for subsequent growth studies. The *Synechocystis* was isolated in pure culture by repeated pour plates of the modified ASP-2 medium plus 2 percent agar.

Growth studies were carried out with a less elegant modification of the test-tube culture method of Myers (*3*). The organism was grown in test-tubes in 10 ml of modified ASP-2 medium in a thermostatted bath at 37°C with 1 percent CO_2 in air bubbling through the tubes. Growth was measured as optical density at 600 mμ, with a Bausch and Lomb Spectronic 20. The optical density was reasonably linear with cell concentration up to an optical density of 1.0. At this density the cell volume was 0.68 mm^3/ml.

Growth is expressed in terms of the specific growth rate k with the dimensions of $\log_{10}$ units per day (*4*), or as the optical density reached in an arbitrary time long enough to show effects of materials which become limiting, but which are present in sufficient amount to allow equivalent growth rates up to an optical density of 1.0. Optical density readings > 1.0 were obtained by diluting the suspension with fresh medium. The B_{12} analogues were diluted from stock solutions (*5*). Table 1 shows the growth response of *Synechocystis* to B_{12}, B_{12} analogues, and methionine.

Experiment 1 shows that the B_{12} requirement is absolute and demonstrates the response to increasing B_{12}. Methionine alone supported only limited growth. Experiment 2 shows the response to various B_{12} analogues. Growth was equally good on all the analogues tested except the two analogues with adenine substituted in the 2-position. With these two compounds the growth rate and the total growth in 40 hours were very low, possibly reflecting a steric effect which causes adenyl B_{12} compounds substituted in the 2-position to be converted only with difficulty to the coenzyme form within the cell.

Several compounds and combinations were tried for sparing action of the B_{12} requirement for *Synechocystis*. Deoxyribosides, S-3 vitamin mix (*2*), choline, sarcosine, RNA, DNA, and DNA hydrolyzed with formic acid allowed no growth. Casamino acids and yeast extract allowed only slight growth at 40 hours. Soytone was somewhat better.

With B_{12} at a saturating level for growth, the addition of Casamino acids or yeast extract caused no increase in growth rate. The addition of Soytone with B_{12} resulted in some inhibition of growth.

Table 1. Response of a species of *Synechocystis* (organism 17a) to B_{12}, methionine, and B_{12} analogues. B_{12} nomenclature is taken from *J. Am. Chem. Soc.* **82**, 5575 (1960). OD, optical density.

Addition	k	OD at 40 hr
Experiment 1		
No B_{12}	0	0.027
B_{12}, 0.05 μg/lit.	1.82	1.52
B_{12}, 0.1 μg/lit.	2.18	3.62
B_{12}, 0.5 μg/lit.	2.10	7.45
B_{12}, 1.0 μg/lit.	2.20	7.70
DL-Methionine, 20 μg/ml		0.147
DL-Methionine, 200 μg/ml		.268
Experiment 2		
No B_{12}	0	0.007
α-(5, 6-Dimethylbenzimidazolyl) cobamide cyanide, 1.0 μg/lit.	2.30	5.09
α-(2-Methyladenyl) cobamide cyanide, 2.0 μg/lit.		0.96
α-(Adenyl) cobamide cyanide, 2.0 μg/lit.	2.36	7.21
Cobinamide, 2.0 μg/lit.	2.20	5.09
α-(5-Hydroxybenzimidazolyl) cobamide cyanide, 2.0 μg/lit.	2.36	6.20
α-(2-Methylmercaptoadenyl) cobamide cyanide, 2.0 μg/lit.		0.27
α-(5-Methyladenyl) cobamide cyanide, 2.0 μg/lit.	2.20	5.53
α-(Benzimidazolyl) cobamide cyanide, 2.0 μg/lit.	2.36	4.95

Various 6-substituted pteridines known to occur in blue-green algae (*6*), including pteridines isolated from *Synechocystis*, were tested singly and together with methionine for possible sparing action. None had any marked effect except 2,6-diamino-4-hydroxypteridine (*7*), which, in the presence of methionine, allowed twice as much growth in 40 hr as methionine alone.

In common with many other marine algae and protozoa (*8*), it appears that marine blue-green algae will also show a requirement for exogenous B_{12}. The red pigmented marine blue-green alga, *Phormidium persicinum*, requires B_{12} (*9*). An examination of the vitamin requirements of other marine blue-green algae recently isolated in this laboratory has shown that more than 50 percent also require B_{12}. Whether this requirement for a vitamin and possibly utilization of other organic materials reflects a greater degree of heterotrophy in marine blue-green algae than in the fresh water blue-green algae presently known remains to be seen (*10*).

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2 February 1961

Synthesis of Uracil under Conditions of a Thermal Model of Prebiological Chemistry

Abstract. Uracil has been formed under thermal conditions which yield other materials of theoretical prebiochemical significance. The chemical evidence for the identity of uracil rests on chromatography, spectrography, color tests, and melting points and analyses of isolated material. The place of uracil in a scheme of biochemical origins, including carbon dioxide as an intermediate, is discussed.

A thermal model of biochemical origins has yielded a network of chemical pathways resembling a flowsheet of biosynthesis (*1*), amino acid copolymers having many of the properties of proteins (*2*), and microspherical units with some of the properties of bacterial boundaries (*1*). Part of the uniqueness of this model lies in the fact that these phenomena emerge in a continuum of physical conditions (*1*). One of the first products in the thermal experiments was ureidosuccinic acid (*3*), an intermediate in the biosynthesis of pyrimidines (*4*). This report describes the direct synthesis of uracil itself under conditions consistent with the other physical details of the model and of its recapitulationist emphasis on malic acid (*5*) which is in turn derivable from simpler compounds (Fig. 1).

In the light of the recent finding that polyphosphoric acid promotes the thermal condensation of amino acids (*1*) and the recognition of the many biological activations by polyphosphate, earlier studies of the reaction of malic acid and urea were extended by testing polyphosphoric acid as a reactive solvent. In the first experiments hypothetical hydrogen acceptors such as nicotinamide were included, but controls showed that the reaction proceeded in the presence of polyphosphoric acid alone. No chromatographic evidence of uracil was obtained in numerous experiments in which ureidosuccinic acid or oxaloacetic acid was included instead of malic acid.

Chromatography was carried out with ascension in one dimension on 3 MM Whatman chromatography paper with butanol-1 saturated with 10-percent urea or butanol-1 saturated with 7*N* NH_4OH. The R_F values of authentic uracil and of one spot was 0.32 in the former solvent and 0.16 in the latter solvent. The spots were identified by fluorescence in the rays from a Mineralight R-51 lamp. Uracil was isolated from a simultaneous series of bar chromatograms by elution with 0.1*N* HCl.

The conditions studied in the synthesis of uracil included temperatures in the range of 100° to 140°C, heating periods of from ¼ to 2 hr, and a range of ratios of polyphosphoric acid (Victor Chemical Co.) to equimolar proportions of malic acid and urea. Uracil was obtained over the entire range of temperature. Increase in proportion of polyphosphoric acid had the most pronounced effect of the factors tried. The variation in one experiment is shown in Table 1. The yield was determined by elution after chromatography and estimation against a standard curve at 260 mμ on a Beckman DU spectrophotometer.

From one of these samples was isolated by bar chromatography 86 mg of white recrystallized (H_2O) uracil of melting point and mixed melting point, 335°C, decrease uncorrected. Calculated for $C_4H_4N_2O_2$: C, 42.85; H, 3.59; N, 24.99. Found (by the Mikroanalytisches Laboratorium, Bonn, Germany): C, 42.84; H, 3.71; N, 24.84.

The uracil synthesized in the presence of polyphosphoric acid had a melting point and mixed melting point with authentic material of 335°C, decrease uncorrected. The Wheeler-Johnson color test with bromine-water and barium hydroxide (*6*) was positive

Table 1. Yields of uracil from varying amounts of polyphosphoric acid with 0.010 mole of malic acid and 0.015 mole of urea at 130°C for 60 min.

Amount of polyphosphoric acid (ml)	Yield of uracil (mmole)
0.5	Negligible
1.0	0.04
2.0	0.06
4.0	0.4
8.0	1.2
16.0	1.4

Ultraviolet absorption spectra of the newly synthesized material in acid, in neutral, and in alkaline solution were identical with the corresponding spectra of authentic material. The infrared absorption in Nujol mull of the new material was also identical with that of authentic uracil.

The thermal formation of urea from the inorganic substances ammonia and carbon dioxide is well documented as an industrial reaction and can thus explain the primordial origins of urea (*7*). Malic acid can be visualized as producible in the primitive context from acetic acid (*8*) which could arise in turn from methane, water, and hydrogen (*9*) or from glycine by radiolysis (*10*) or in other ways (*11*). Although the presence of carbon dioxide in the original atmosphere has been a controversial issue (*11*), it should be recognized that carbon dioxide would probably become available as the result of reactions of other compounds—for example, Miller has found substantial proportions of carbon dioxide in a sparked atmosphere which contained none at the outset (*9*).

The results reported lead to a revised thermal flowsheet, as shown in Fig. 1. The now enlarged picture of prebiochemical reactions, including synthesis

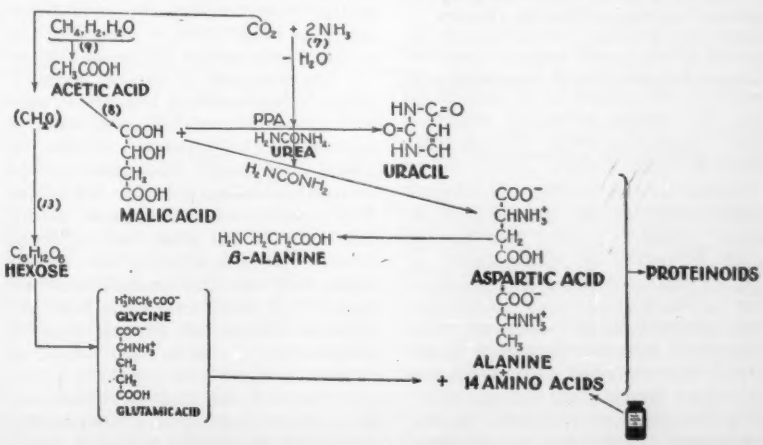

Fig. 1. Flowsheet, predominantly thermal, of suggested prebiochemical reactions. Unnumbered reactions are documented in (*1*).

of the nucleic acid component and of polymers with many properties of proteins (*2*), is consistent with a unified concept of the origin of biochemical pathways in a predominantly phosphoric medium (*14*).

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3 February 1961

Quantal and Graded Analysis of Dosage-Effect Relations

Abstract. Loewe's recommendations regarding treatment of stimulus-response relations are criticized. Conditions are described where quantal analysis is justified. Loewe's interpretation of his graded analysis must be modified in the light of the fact that response curves for individuals frequently cross. Superior lines of attack on the problem are suggested.

Loewe (*1*) has examined the logical consequences of an investigator's decision whether to treat responses to drugs as graded (ordinally scaled) or quantal (dichotomized). He concluded that the two analyses bring out different relationships, and that since the chief concern in most experiments is to determine the relation of response strength to stimulus strength, the quantal analysis is inappropriate. Especially because the conclusion, if valid, would apply widely in behavioral and biological research, it requires close scrutiny.

Loewe regards as unsatisfactory analyses which discard an appreciable amount of important information from the data, and this view is beyond dispute. Two situations, however, may be distinguished: either differences in response strength all along the scale are important to the investigator (as Loewe assumes), or there is some response level E_0 of special significance, such that attaining it or not attaining it is far more important than differences in response level elsewhere on the scale. The second case is not infrequently encountered. In personnel selection a dichotomous criterion of job performance appropriately represents utility to the firm, if differences between satisfactory men and those who must be discharged are far more critical than differences in output among the satisfactory men (*2*). In a tryout of advertising, the consumer's decision to buy or not to buy the product should sometimes be studied without taking into account degrees of interest in the product above or below this point. Surely there are drugs (for example, anesthetics, insecticides) where a certain level of response marks the transition from useless to useful effect. There may be theoretical as well as utilitarian reasons for preferring to quantize data; Estes (*3*) argues for analyzing learning in terms of the appearance or nonappearance of a response rather than in terms of a scaled measure such as latency.

If there is a critically important E_0, the information desired is the proportion of subjects giving a response equal to or greater than E_0, as a function of stimulus strength (dosage, D, in Loewe's case). There may be more than one critical E; if so, more than one probability function can be plotted.

Even if gradations of response are important, Loewe's recommendations require re-examination. His mathematical model consists of a surface representing effect E as a monotone increasing function of dosage D and individual tolerance T. In the model, T is not clearly defined; we know only that the individual's responsiveness is expressed as a percentile relative to others under study. To summarize the surface conveniently, Loewe would use D,E cross sections (T constant at 50, 16, and 84, or other such values). Loewe then describes an experiment whose data are coordinated with this model. The proposed single-dose experiment consists of drawing random samples and giving to each sample a different dosage. This produces a distribution of E for each D, which can be converted into a curve representing cumulative probability p_E as a function of E. The curves for various D form a surface. Loewe identifies p_E with T, though one is a group statistic and one is a constant associated with the individual; hence he identifies the D,E,p_E surface with the D,E,T model. He takes cross sections with p_E constant as the desired summary curves.

Loewe's model appears to be overly restrictive. His surface represents actual data only if all individuals having a certain tolerance T have the same D,E curve, within the limits of experimental error. This can occur only if the set of curves for all individuals is disjoint, that is, if no two curves cross each other within the range of D under study. When curves are disjoint, the only fault in his recommendation is that he preserves too little information to satisfy the person for whom some E_0 is of prime importance.

As a matter of fact, however, learning curves, drug-response curves, and so forth, for individuals often cross (*4*, *5*). Some measure of the individual (for example, strength of response at some arbitrarily chosen dose) may be used to represent T, but for every T there will be numerous D,E functions, and a distribution of E against D, not a curve, will be obtained. Only when curves are disjoint is it correct to identify p_E with T. A single-dose experiment does not permit a test of disjunction. If such an experiment is performed, and if utility considerations make gradations of response important, Loewe's analysis is an acceptable method of summarizing the distribution of E as a function of D even though it probably does not represent the relation of E to D for constant T.

Wherever the risk of unwanted order effects can be disregarded, it is much more informative to carry out an individuals-times-levels experiment in which several points on the curve for each individual are determined by successive dosings. If the data support the assumed disjunction of curves, one can give Loewe's analysis the strong interpretation he proposes. If they do not, a more powerful analysis should be sought. One possibility is to divide persons into groups such that the set of curves within any group is disjoint, after which Loewe's analysis can be applied. Even more powerful is the technique of establishing a limited number of prototype curves and describing each individual's record in terms of one curve or a combination of them (*5*). Such techniques for recognizing individual variation in shape of curve as well as differences in threshold level are still in an early stage of development.

Loewe's argument is consistent only if we accept his hidden assumptions: that no level of effect is especially important, that D,E curves for individuals are disjoint, and that a single-dose ex-

periment is to be used. If curves intersect, as is likely, neither his experimental design nor his analysis is powerful enough to depict "without loss of faithfulness . . . the essentials of the . . . *D,T,E* relationship." As a step in the direction of taking individual differences into account, it is a useful procedure when interpreted somewhat differently than Loewe suggests. His case for rejecting quantization as a matter of principle does not appear to be well founded.

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27 January 1961

As is clearly stated in the introduction of my paper (*1*), the sole purpose was to examine the general question whether or not the task of quantifying the relation between dose (*D*) and effect (*E*)—which is appropriately solved by presenting the change in *E* with increasing *D* in a so-called "graded-response" *D,E* curve—can also be solved by presenting, in a so-called "quantal-response" curve, the change, with increasing *D*, of a third magnitude (called *T* in my paper, p_E in Cronbach and Gleser's), the percentage of test individuals attaining or exceeding a certain single preset *E* level (*2*). The question was answered—to the negative—by linking the three magnitudes concerned in a three-dimensional coordinate system with an *E* coordinate rising over a rectangular *D,T* plane, by then forming a *D,T,E* space surface under use of values freely chosen but compatible with experience, and by demonstrating that, since any quantal *D,T* curve lies in a horizontal, any graded *D,E* curve in a vertical plane, they intersect rectangularly "and never the twain shall coincide."

This simple demonstration of the nature of the relation between three basic magnitudes required no experiments, and in fact my paper contains no experimental data nor does it describe, prescribe, recommend, or touch any practical, technical, methodical, or procedural matter. From the fact that in Cronbach and Gleser's reply this model demonstration is called an "experiment" one may consider it possible that the authors have misunderstood the scope and essence of my article. It is food for such supposition that one encounters nowhere in their reply direct and specified objections against my completely theoretical demonstration or against conclusions drawn from it, or against both, but that instead their discussion begins with the extensive description of an "experiment" allegedly encountered in my paper ("Loewe then describes an experiment . . ."), the details of which then form the starting-points and targets of their objections. Even if it were appropriate to call my demonstration an experiment, the experiment described by Cronbach and Gleser is in many important respects unrelated to the object of my demonstration; it is *their* experiment, not mine.

It is true that my *D,T,E* model, by definition (and an extensive, unmistakable definition) is identical with what they call their *D,E,*p_E surface—which makes any objection to my "*T*" a mere quarrel about *names*. However, their experiment also deals with entirely new relations such as the enigmatic "p_E as a function of *E*." Most, and the most grave and intricate, objections are directed against the "single-dose experiment" character of "my" experiment. This obviously refers to the use of figures and curves obtained from single individual test objects rather than from groups of such. And indeed, actually all of the subsequent discussion of Cronbach and Gleser's reply is focused to the inadequacies, dangers, and fallacies of such "single-dose experiments." Now quite incidentally, although any such questions of experimental procedure are irrelevant in our analysis of basic relations, a technical subject was touched, quite at random, *once* in my article: by mentioning in reference to the *D,T* curves that the percentile distribution values are customarily (and, of course, necessarily) "derived from single-dose *group* experiments." And just as customarily in this statistically minded age the values for *D,E* curves come from single-dose *group* experiments. The authors' "anti-individual" campaign cannot possibly be due to misinterpreting "single-dose group" into its contrary; at any rate, such a campaign is directed to the wrong address, so much more so as in pharmacology intra-individual variation from one test to the other is a well-known, much discussed, and well-heeded phenomenon (see, for example, *3*). Unfortunately, both the constructive suggestion made by Cronbach and Gleser and their promise to contribute to future developments refer only to problems of individual variation, important in *their* experiment but irrelevant to my analysis of fundamentals and my two conclusions submitted: (i) that in the treatment of the problem in question, namely, how to obtain information on intensity of *E* as a function of *D*, the quantal-response *D,T* curve cannot replace the graded-response *D,E* curve, and (ii) that multiple *D,T* curves offer an important tool for statistically supporting and refining the graded-response information.

It is a distressing paradox that, in the summarizing sentence of Cronbach and Gleser's reply, this championing of mine for an appropriate application of quantal-response studies in graded-response investigations is called Loewe's "case for rejecting quantization as a matter of principle."

For those who still adhere to the belief that a quantal-response curve is equivalent to a graded-response curve in depicting the *D,E* relation, my image of the level path of the student of the former and the up-hill climb of that of the latter should perhaps be thrown into somewhat bolder relief: In college towns, such a strictly level promenade built halfway up along a hillside for the convenience of elderly scholars is often named "Philosophers' Lane"; should the meditating philosopher insist that in walking there he gained altitude, one would call that an illusion.

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13 February 1961

Chromosomes of Lemurine Lemurs

Abstract. A wide variation in chromosome number and morphology was observed among different species and subspecies of lemurine lemurs. Comparative karyotype analysis indicates close phylogenic relationships and strongly suggests that chromosome structural rearrangements may have played an important role in the evolution of this group of primates.

The lemurs, a unique group of the most primitive primates, the Prosimiae, have survived millions of years. Their distribution has been limited to the island of Madagascar (the Malagasy Republic), and they are now threatened with extinction. Although the diversity of forms has been noted since before

Table 1. Mitotic chromosome numbers of lemurs. M, male; F, female.

Name of lemur	Animals studied (No.) M	F	Diploid No. (2n)
Division I			
H. griseus olivaceus (grey gentle)		1	58
H. griseus griseus (grey gentle)	1	1	54
Division II			
L. mongoz (mongoose)	2	3	60
L. fulvus rufus (red-fronted)		2	60
L. fulvus albifrons (white-fronted)		1	60
L. catta (ring-tailed)	2		56
Division III			
L. sp. nov. (6) (brown)	1		52
L. fulvus fulvus (brown or fulvous)	1	1	48
L. variegatus (ruffed)	1		46
L. macaco (black)	1	1	44

Darwin's time, the cytology of this group was virtually unknown until recently, when a description of the chromosomes of the black lemur (*Lemur macaco*) was reported (*1*). In fact, only five other prosimian species have been examined cytologically (*1*).

We have made a study of the somatic chromosomes of a number of species and subspecies of lemurs, all belonging to the subfamily Lemurinae of the family Lemuridae. Skin biopsy specimens were obtained and sent to this laboratory, either directly from Madagascar or from New Haven, Connecticut, where some of the captive animals are kept (*2*). Chromosome analyses were made on cells grown in culture. The cell culture and cytological techniques employed were essentially the same as those for human materials and have been previously described (*3*).

The diploid chromosome numbers of the group of lemurs so far examined are listed in Table 1. Variations in both the number and morphology of the chromosomes exist within a single genus and even within hitherto recognized taxonomic species (*4*). The length of metaphase chromosomes varies from approximately 0.3 to 14 μ. The X chromosome is medium-sized and either rod-shaped (acrocentric) or J- or V-shaped (metacentric); the Y is invariably acrocentric and is the smallest in the complement. In some species (*Hapalemur griseus, Lemur variegatus,* and *L. macaco*) two distinct classes, macro- and microchromosomes, are present; in the other species there seems to be no discontinuity in the range of chromosome lengths. A cell from a male brown lemur (*L. fulvus fulvus*, $2n = 48$) is shown in Fig. 1, which shows variations in the size and shape of the chromosomes. In this species the X is an acrocentric macrachromosome, and the Y is the smallest element.

There is a direct relation between the total number of chromosomes and the number of acrocentric chromosomes. For example, in *L. mongoz* ($2n = 60$) all but two pairs of autosomes are acrocentrics, whereas in *L. macaco* ($2n = 44$) only ten pairs of autosomes are acrocentrics. In addition, detailed karyotypic comparisons of chromosome lengths, position of centromeres, secondary constructions, and other morphological features indicate similarities between certain chromosomes of different species or subspecies. The presence of these "marker" chromosomes suggests homology, thus denoting close interspecific relationships. In many instances, karyotypic differences between any two particular species can be interpreted as the result of one or more chromosome structural rearrangements such as reciprocal translocation and pericentric inversion. A Robertsonian-type of chromosome evolution (see *5*) has probably played a significant role in this group of primates. On the basis of these cytological findings, the lemurs examined to date may be subdivided into three tentative divisions (Table 1). Species within each division seem to be closely related, and stepwise interrelationships are easily traced. Nevertheless, interdivision resemblances are not uncommon. On the other hand, it should be pointed out that the lemurine lemurs as a group exhibit sufficient karyotypic differences from other prosimian primates studied to obscure the interfamily relationships (*7*).

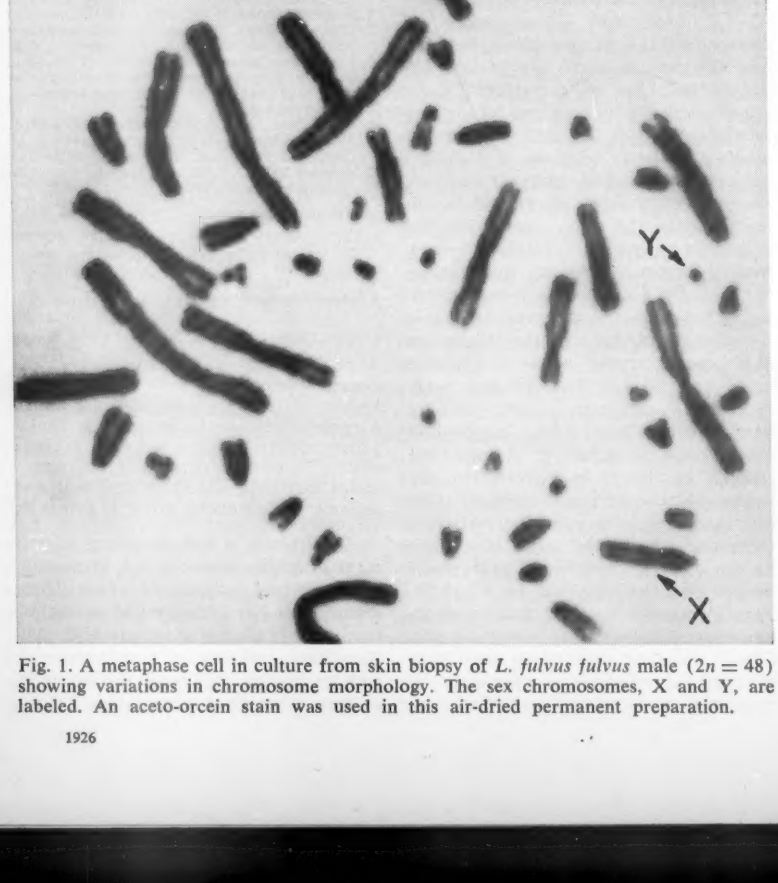

Fig. 1. A metaphase cell in culture from skin biopsy of *L. fulvus fulvus* male ($2n = 48$) showing variations in chromosome morphology. The sex chromosomes, X and Y, are labeled. An aceto-orcein stain was used in this air-dried permanent preparation.

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7. A detailed cytological description of the lemurs and a proposed phylogeny is in preparation.

16 January 1961

Evoked Cortical Activity from Auditory Cortex in Anesthetized and Unanesthetized Cats

Abstract. Evoked electric responses of the auditory cortex of unanesthetized cats were studied by averaging techniques which enable us to detect responses in high-level background activity without sacrificing bandwidth. The results suggest that previous studies on evoked responses from behaving animals that are based upon ink-written electroencephalographic displays were chiefly concerned with a set of response components that differ from the classical evoked potentials in anesthetized animals.

In recent years the use of implanted electrodes has made possible the study of electric activity in the nervous system of unanesthetized behaving animals (*1*). Given the range and variety of recordable phenomena, it is not surprising that some confusion has arisen in relating recordings of electrical events in anesthetized and unanesthetized animals. During the past quarter-century an extensive literature on the so-called "classical" evoked responses from the sensory cortical areas of anesthetized preparations has accumulated. This knowledge should provide a helpful anchor when one seeks to explore the new and exciting possibilities of recording electrical events that relate to an animal's behavioral responses to sensory stimuli.

The "classical" evoked response from the auditory cortex to a click consists of a relatively stable surface-positive deflection followed by a more anesthesia-sensitive, surface-negative deflection. These two deflections are quite stable in latency but exhibit a considerable amount of amplitude variability. Given the temporal characteristics of these deflections (see Fig. 1), the customary ink-written electroencephalographic records (which have a bandwidth equivalent to a 60-cy/sec low-pass system) do not give an adequate display of these fast events (Fig. 2). In order to obtain a faithful and yet stable representation of evoked responses, the background activity can be selectively removed by the use of averaging (*2*).

The present report concerns the application of averaging to the study of evoked responses with implanted electrodes in the awake cat. In this instance, a Teflon-insulated nichrome electrode was implanted in the auditory cortex 6 months before the recording date. During recording, the cat was placed in a sound-proof chamber while clicks were delivered from a loudspeaker. Since cortical response patterns depend upon the state of the animal, we tried to put the animal into a more or less uniform state during each run. Responses were first obtained while the animal was awake.

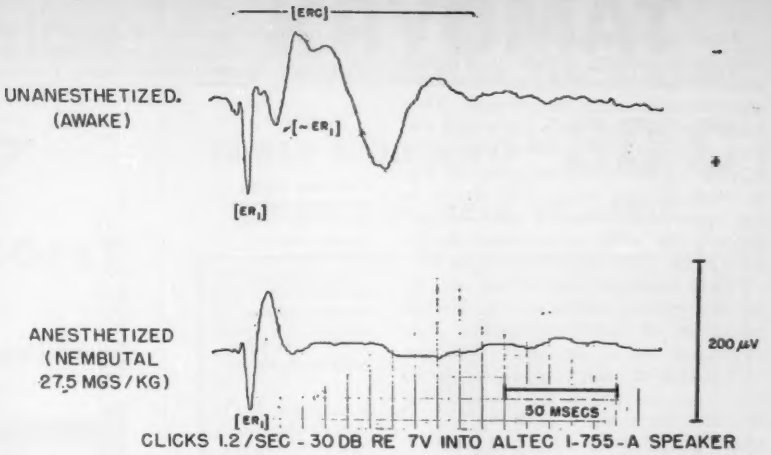

Fig. 1. Average of 256 responses from the auditory cortex of a cat in the awake state (upper trace) and in anesthetized state (lower trace). Similar results have been obtained from three other cats. The difference in the amplitude of the ER_1 component in the top and bottom traces lends itself to several interpretations. The wave form of the "slower" components of the ERC may have been affected by the low frequency cutoff of our recording system.

Nembutal was then administered intraperitoneally, and an hour later a second run was made under comparable stimulus conditions. During each run the electric activity from the implanted electrode referred to a frontal lead was recorded on magnetic tape. The bandwidth of the entire recording system covered the range from 1.5 cy/sec to 5 kcy. The recorded data were processed on the ARC-1 computer (*2*). Further details on the relation of average responses to individual responses are given in (*3*). The results of averaging are shown in Fig. 1.

Even a cursory examination of these findings points to the need for clarifying the term "evoked cortical response," if one attempts to cover records from both anesthetized and awake animals. In awake cats, the pattern of activity time-locked to the stimulus has at least several components. Under these circumstances it might be more appropriate to designate this pattern as an evoked response complex (ERC). A comparison of the top and bottom traces in Fig. 1 indicates that the one clearly common element is the early surface positive deflection of the classical evoked response. Even the later surface negative component is sufficiently modified, so that it can no longer be isolated in the ERC. Thus it might be convenient to designate the common element as the ER_1 and to withhold identification of later components until they have been examined more closely. Beyond this our state of knowledge warrants little more than a gross label of that part of ERC that is not ER_1 as $\sim ER_1$.

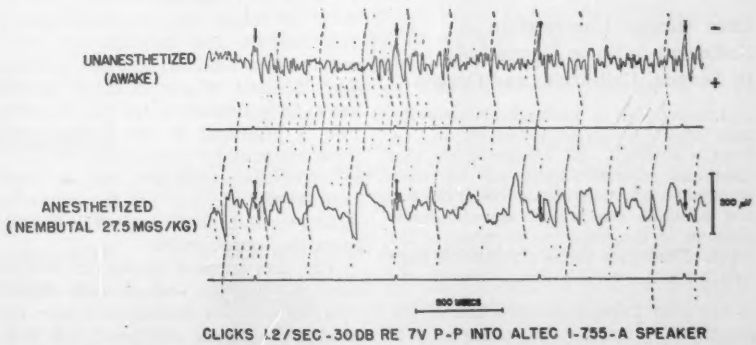

Fig. 2. Ink-written electroencephalographic records that belong to the same set of taped data from which the average responses in Fig. 1 were computed. The time markers under each record indicate the occurrence of clicks; the arrows identify the response components that are visually most easily detectable. Upward deflection indicates negativity of auditory cortex referred to frontal bone.

Most of the studies of electric responses to sensory stimuli in behaving animals have been based on ink-written electroencephalographic records. They have thus dealt predominantly with that part of the ERC that we suggest be labeled $\sim ER_3$. An examination of Fig. 2 bears out this point. The arrows in the upper trace point to what are apparently the $\sim ER_3$ components; the arrows in the lower trace indicate the ER_3.

These results do not in any way invalidate existing studies on the electric correlates of conditioning. Our data emphasize that the behavior of ER_3, as well as that of other components of the ERC, can be studied as a function of stimulus parameters and organismic states, provided the ERC is adequately displayed (*4*).

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* Present address: University College Hospital, London, England.

6 December 1960

Clay Mineral Composition of Sediments in Some Desert Lakes in Nevada, California, and Oregon

Abstract. X-ray analyses of some Recent desert lacustrine sediments in Nevada, California, and Oregon show that illite and montmorillonite are the most abundant clay minerals and that chlorite and kaolinite are present in subordinate amounts in the sediments of many of the lakes. These clay suites are derived from source rocks.

The clay mineral composition of the less-than-2-μ-fraction of nineteen Recent desert lacustrine sediments from Nevada, California, and Oregon yields data to extend an earlier study of the clay minerals in playas of the Mojave Desert, California (*1*). The sediments of the lakes studied have a wide range in chemical character, from weakly saline to very saline, in which deposits of calcium and sodium salts (sulfates, halides, carbonates, borates, and others) are found. A wide variety of composition is found in the source rocks surrounding the basins, and sediment derived from the same kind of source rocks are deposited in lakes with different chemical environments. By comparing the clay composition of the detritus coming from the source rocks with the clay suite of the lake sediment, conclusions concerning the diagenesis of clay minerals in the continental saline environment can be made.

Table 1. Relative abundance of clay minerals expressed in parts in ten of the less than 2-μ fraction. The symbol (d) indicates that the clay mineral is partially degraded. Amorphous material abundance: A, abundant; M, moderately abundant; T, present in minor or trace amounts.

Location	Illite	Montmorillonite	Chlorite	Kaolinite	Amorphous material
		Nevada			
Columbus Marsh	4	2	1	3	M
Fish Lake Marsh	5	3	1(d)	1	T
Silver Peak Marsh	3(d)	6	1(d)		M
Rhodes Marsh	4(d)	4	1(d)	1	T
Teels Marsh	4	3	3		T
Carson Sink	4(d)	3	2	1	T
4-8 Mile Flat	5(d)	2	2	1	T
Pyramid Lake	5(d)	3	1	1	M
Smoke Creek Desert	4(d)	4	1	1	T
Winnemucca Lake	5(d)	3	1	1	T
Black Rock Desert	5(d)	3	1	1	T
		California			
Borax Lake	6(d)	4			T
Deep Spring Lake	8	1	1(d)		T
Middle Alkali Lake	4(d)	6			M
Mono Lake	5	2	2	1	M
		Oregon			
Abert Lake	3(d)	7			A
Silver Lake	2(d)	8			A
Summer Lake	3	7			M
Harney Lake	3(d)	6	1		M

The relative abundance of clay minerals, determined by x-ray methods, in the sediments studied is given in Table 1. Illite and montmorillonite are the most abundant clay minerals found, and chlorite and kaolinite are present in minor amounts in many samples. The illite and chlorite of many samples are slightly degraded, and the degradation is produced in the source rocks predominantly by weathering. No clearly significant variation can be seen between the clay suite of the lacustrine sediments and the clay minerals derived from the source rocks.

The data support the earlier conclusion (*1*) that the sodium and calcium continental saline environment does not produce diagenetic changes in clay mineral composition. This conclusion does not apply to those saline environments where magnesium and potassium activity is high. The clay data obtained from a study of the Saltair core from Great Salt Lake, Utah (*2*), and from cores in several basins in California also suggest that the clay minerals usually are not effected by diagenetic processes in continental (sodium and calcium) saline lakes (*3*). Several lakes contain sediment rich in material amorphous to x-rays, and the lakes which contain the sediment most amorphous to x-rays are found in areas where Recent vulcanism is extensive. The montmorillonite content of most of the samples likewise is related directly or indirectly to ash. The The entire region of the study has been an area of active volcanoes through much of Tertiary time, and source rocks of the basin sediment are rich in volcanic products. Although the evidence is inconclusive, it is believed that montmorillonite is forming from glass in several of the basins studied, almost certainly in Abert Lake and Silver Lake, Oregon.

JOHN B. DROSTE
Department of Geology, Indiana University, Bloomington

References and Notes

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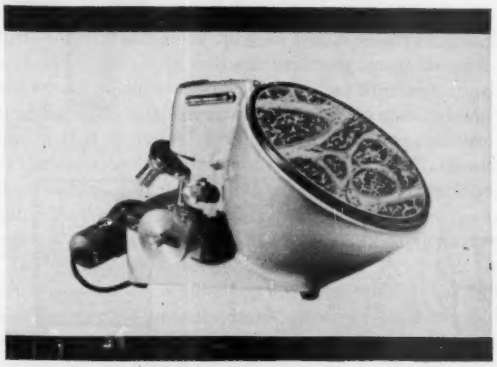

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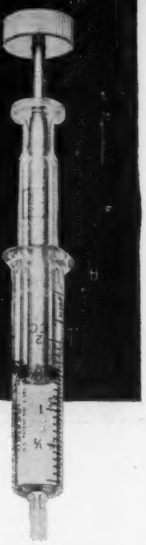

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Letters

Population Density and Growth

Last November an article by Von Foerster *et al.* appeared in *Science* [**132**, 1291 (1960)] in which it was contended that in about 66 years the growth rate of the world's human population would approach infinity, and that our descendants would be squeezed to death. Most demographers who read this article thought that it was intended as a joke, but the reaction of the press and a careful rereading indicate that the principal contentions were presented and widely received as serious.

The prediction of an infinite growth rate in A.D. 2027 is obtained in effect by fitting a curve to estimates of growth rates in world population from the beginning of the Christian era until the mid-1950's. A rectangular hyperbola provides a rather good fit to these estimates through 1957, and it is the extrapolation of this hyperbola that yields the approach to infinity for the rate of growth early in the next century. The choice of the rectangular hyperbola is dictated by a differential equation expressing the general notion that the growth rate has increased as population density has increased. The authors note that the familiar logistic curve may be derived from a differential equation expressing a *diminishing* rate of growth as density increases. They say that such a negative relationship is plausible for a population whose elements must compete with each other for limited resources available in nature. However, they conjecture that *Homo sapiens* has the capacity to form coalitions against nature, to be more efficient in the struggle to control nature as density increases, rather than less efficient. In other words, they visualize man as cooperating in extracting more from nature rather than as competing for a fixed endowment of resources. This view leads them to expect increasing growth rates with increasing density, and serves, as was stated above, as the rationale for a differential equation which in turn leads to the rectangular hyperbola as the appropriate curve to express the annual percentage increase in population.

This whole argument will not stand critical scrutiny. The following defects are the most conspicuous:

1) It is not true that population growth rates are positively associated with density, nor even that population growth rates are positively associated with man's control over nature. The populations currently residing in areas with a density of less than 100 persons per square mile are growing at more than twice the pace of populations residing in areas with a density of more than 500 persons per square mile. In other words, in the world today the association between growth and density is negative, not positive. Similarly, the countries which have come farthest in technology, in communication, and in what in any view should be considered control over nature have growth rates that range from the slowest in the world to about average, while the fastest growth rates are found in such areas as Malaya, Taiwan, Ceylon, Mexico, and Brazil—countries which certainly cannot be considered in the same class with the United States, Western Europe, or the Soviet Union in their control over nature.

2) Von Foerster and his co-authors neglect the causes of accelerated population growth in the world. Growth has accelerated wholly because of a reduction in mortality, because of a lower world death rate. There are no grounds for believing that there has been any upward trend in the world birth rate.

3) There are clear limits, moreover, to the growth rate that can be attained through reducing mortality rates. In fact, if mortality were reduced to zero and everyone lived forever, the growth rate of world population would initially be exactly equal to the world birth rate, estimated by the United Nations as 3.9 percent. In the long run, immortality would produce a somewhat *lower* growth rate than this as the proportion of population above childbearing ages became larger. In short, the extension to its logical extreme of the trend which has been wholly responsible for faster growth in the world population would lead to a growth rate of less than 4 percent. There are also biological limits to increases in fertility in the improbable event that these occur. If every woman were married by age 15 and bore children at a rate equaling the highest observed in any large human population, and if everyone were immortal, the growth rate would still be somewhat less than 6 percent. In fact, rates much above 6 percent cannot be achieved as long as childbearing is restricted to the ages between 15 and 50 and the period of gestation remains 9 months.

Another well-known fact that this article neglected was that the development of better communication, of better education, and of better control over nature has led universally to *declines* in the birth rate. One might say that in extending his control over nature man has learned to exercise rational control over the consequences of his own nature. There is not a single country in the world which has achieved a high degree of industrialization that does not have birth rates very substantially lower than those prevailing before industrialization. Included among the countries where the birth rate has dropped sharply are the Soviet Union, Japan, the United States, Canada, Australia, New Zealand, and all of the countries of Europe except Albania (not yet industrialized). In short, the most superficial examination of population trends makes it clear that if man *does* continue to extend his technology, the expected result will be a decline in birth rates in those parts of the world where they are now highest.

The especially rapid acceleration of growth in the mid-20th century has resulted from technological advances in medicine and public health. Among the most striking and far-reaching developments have been the antibiotics and the residual insecticides such as DDT. Developments of this sort have made it possible for countries which have not yet adopted modern technology in their economies to enjoy a very substantial measure of control over infectious disease. Thus Ceylon, which has undergone very little in the way of fundamental economic change, has cut its death rate by more than 50 percent since 1945. This situation gives the clearest urgency to programs for speeding the economic development of these areas, both to keep production expand-

ing at least as rapidly as population and to provide the basis for the social and economic changes which will bring births under voluntary control.

These questions are among the most serious facing the world today, and to give wide currency to such clearly nonsensical ideas as an infinite birth rate and Doomsday in A.D. 2027 contribute nothing to their solution.

ANSLEY J. COALE
Office of Population Research,
Princeton University,
Princeton, New Jersey

Essentially three arguments against our "Doomsday" thesis are presented in Coale's letter. They are: (i) nonsensicality for a parameter describing a finite physical system to approach infinity; (ii) misrepresentation of relationships between population growth rates, population densities, and technological status; and (iii) natural limitations of growth rates.

It is easy to dispose of these arguments.

1) In answering similar objections raised earlier we have tried to point out [*Science* **133**, 943 (1961)] that such singularities are usually interpreted as an indication of the system's instability in the vicinity of this singularity. In other words, a system may undergo drastic changes—for example, evaporation, rupture, disintegration—when passing through this critical state. Four examples were given, and references were cited. If this does not suffice, we recommend consulting the nearest physicist, who—we are convinced—will be glad to assist in overcoming this conceptual difficulty.

2) It is true that in many instances in various regions an inverse relationship between population density and population growth, or population density and technological status, may prevail, as Coale shows in numerous examples. However, an equal number of cases could be cited where the relationship is a direct one—for instance, if one compares the population density, growth rate, and technological standard of the population in New York with that, say, of the population in the Matto Grosso, or makes the same comparisons for the people of Japan and the people of Tierra del Fuego. Since our expressions for global population growth were derived for mean values over at least one generation—as explicitly stated at several points in our original article—it is obvious that our simple Eq. 11 is neither intended, nor able, to account for minute local and temporal fluctuations. On the other hand, if we apply "Coale's law" of the inverse relationship of population density with growth rate and technological know-how—as suggested in his letter—to the development of the human population as a whole over the last couple of millennia, we arrive at the peculiar conclusion that either Stone Age man was a technological wizard who carefully removed his technological achievements so as not to upset his inferior progeny, or that —if he was at the level at which most of us believe he was—our population dwindled from a once astronomical size to the mere three billions of today. Since we are neither archeologists nor demographers, we must leave the decision between these alternatives to the experts.

3) Four natural limits for growth rate are cited by Coale: period of gestation, onset and duration of fertility, and death. While Coale treats the first three physiological quantities as fixed quantities, unalterable through technological progress, and thus gives the endocrinologist an undeserved vote of

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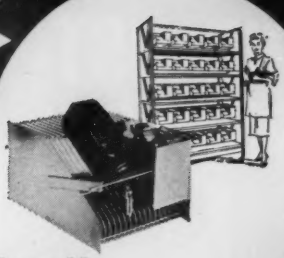

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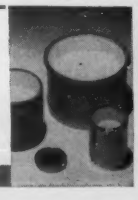

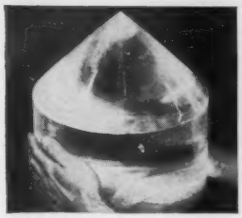

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no confidence, he puts his bets on the gerontologists, who, he theorizes, will eventually solve the problem of immortality; that is, the mean life span of man, t_m, will approach infinity. Noting with pleasure that Coale does not consider such a hypothesis nonsensical, and accepting for the sake of a fair argument his conjecture of an invariable global birth rate γ_0 throughout the history of man, we are now in a position to estimate man's life span as a function of time. Invoking our Eq. 7, which gives the productivity a (defined as $\gamma - 1/t_m$) in terms of dooms-time τ, we have

$$t_m = \frac{\tau}{\gamma_0 \tau - k}$$

Today, $\gamma_0 = 0.039$ per year (U.N.), $\tau = 67$ years, and with $k \approx 1$, the global mean life expectancy becomes 42 years. This seems to be a fair approximation (*1*) of present-day estimates ($40 \leqq t_m \leqq 45$) (*2*). This encourages us to take a step further and to determine the time when undertakers will be out of business. For $\tau = \tau^* = k/\gamma_0$, t_m approaches infinity, and when we use values as above; $\tau^* = 25.4$ years. In other words, around the spring of A.D. 2001 the "Perma-Life" pill will be on sale, and thereafter the population will grow merely according to an exponential.

Let us investigate what happens under these conditions, around doomsday $\tau \approx 0$, or $t =$ A.D. 2027. Expressing γ_0 in percentage per year, we have with our Eq. 11 and the suggested exponential the number of people N_D at doomsday:

$$N_D = 5.10^9 \gamma_0$$

and the annual population increase at that time

$$\left(\frac{dN}{dt}\right)_D = 5.10^7 \gamma_0^2$$

With Coale's permission, we take $\gamma_0 \approx 6$ percent and obtain the frightening result that in 67 years the world population amounts to 30 billion souls, and that this population produces in that year 1.8 billion immortal babies. In other words, a number of people equal to the entire world population in 1927 will thus be added in a single year.

A reader unfamiliar with demographic numerology should be cautioned that almost nothing changes in these figures if the immortality assumption is dropped, as Coale has shown elsewhere (*3*). However, drastic

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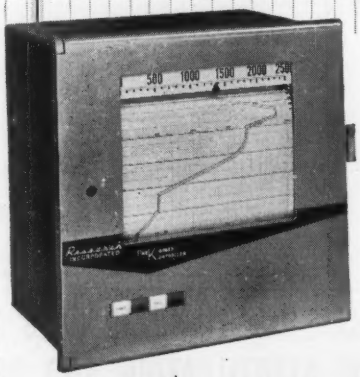

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changes for the worse can be predicted if medicine should succeed in making only small advances in extending years of fertility, or if our culture should promote a shift to slightly earlier marriages.

We leave it to the poets to describe this terrestrial inferno, and to the anthropologists, sociologists, and psychologists to give their verdict on the stability of our human condition under such circumstances.

We hope that in this exchange of arguments about a few side issues the reader has not lost track of the real issue at stake—namely, whether or not the time has come when man must take control over his fate in this matter and attempt to launch perhaps the most ambitious, most difficult and most grandiose enterprise in his entire history: the establishment of a global control mechanism, a population servo, which would keep the world's population at a desired level. It is depressing to discover the complete lack of comprehension of the dimension of this problem in a leading demographer, who seriously believes that sending gadgets to the "underdeveloped" will quickly alter their ancient cultural habits. He even dares to cite the recent fast decline of the birth rate in Japan. Indeed, there were only three gadgets involved: a curet, a dilator, and penicillin. With these, in the years 1950 to 1956, about 12 million successful abortions were carried out, reducing the live-birth rate in Japan from 3 percent to 1.8 percent (*4*) and thus demonstrating—after Coale—the comforting principle of reduced birth rates with improved technical skill.

The tragic error made by Coale and others of his school of thought is to insist that mankind has to be treated—like fruit flies—as a set of independent elements whose only properties are their fertility and their mortality schedules, which are established *ad hoc* by looking into census figures. Very little indeed can be expected from such an approach, which not only fails to describe the past of the human population beyond a dozen generations (*5*) but also is invalid for projecting population trends over such short an interval as only one generation (*6*). Therefore, it does not come as a surprise that recognition of an obvious trait in man—namely, his capacity to form coalitions, the ability of two men *jointly* to do things which the two independently are never able to achieve—immediately leads to expressions which adequately describe human

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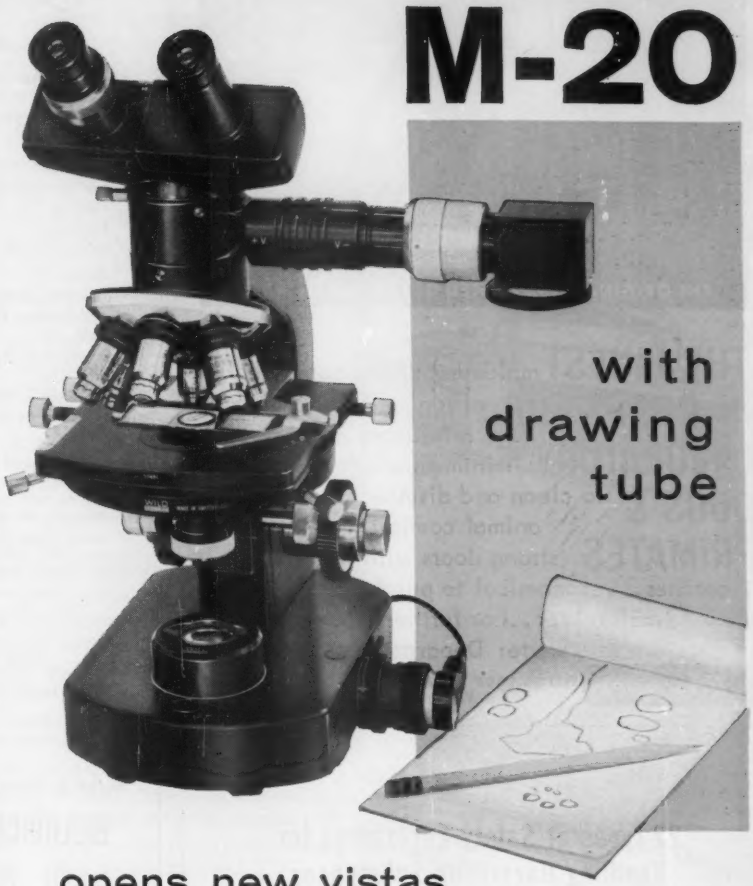

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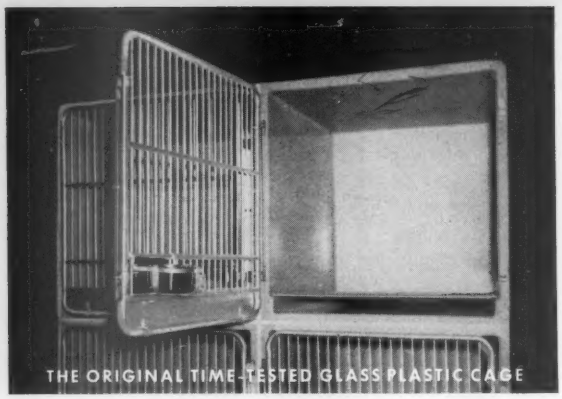

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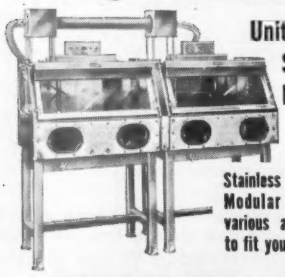

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population growth over several hundred generations, from the prehistoric past up to today. As we pointed out, the process which governed the growth rate for a couple of thousand of years and which is still acting today, exhibits a most dangerous intrinsic instability, which is now—so to say—around the corner. It is clear that this process has to be interrupted, and, as we believe we have shown, to suggest stepped-up industrialization is to propose to put out a fire with gasoline.

The real problem is that today we have to prepare each single member in a family of 3 billion to face soon a decision—namely, either to persist in enjoying his children and to pay for it by having no more than two and remaining mortal, or to reach for individual immortality and remain childless forever. In 20 years, of course, 4 billions will have to make this decision.

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References and Notes

1. A better approximation is of form $t_m = (1/\gamma_0) + (1/\pi\gamma_0^2)$, with $\gamma_0 = 0.034$. Compare with E. S. Deevey, Jr., *Sci. American* **203**, No. 3, 200 (1960). This, however, results in a slowly increasing birth rate.
2. H. F. Dorn, in *The Study of Populations*, P. M. Hauser and O. D. Duncan, Eds. (Univ. of Chicago Press, Chicago, 1959), p. 455.
3. A. J. Coale, in *Trans. Intern. Population Conf.* (Vienna, 1959), pp. 40–44.
4. P. H. Gebhard, W. P. Pomeroy, C. E. Martin, C. V. Christenson, *Pregnancy, Birth and Abortion* (Harper, New York, 1958), p. 219.
5. W. E. Howland, *Science* **133**, 939 (1961).
6. Official population projections for 1980 made in 1950 are now revised by adding a mere 500 million people to the old estimate. Compare *Population Bull. No. 1* (United Nations, New York, 1951), pp. 1–3, and *ECOSOC Report, Time* **77**, No. 16, 31 (1961).

Reciprocal Disarmament

I have not as much hope today for the *advancement* of science as I have fear for its *retrogression*, or even extinction—a process in which (in my opinion) many scientists and technologists are aiding, either by their indifference to the diversion of scientific discovery to purposes of destruction or by active furtherance of such diversion. How sadly must the shades of Newton and Boyle, Thomson and Rutherford regard the great edifice they founded, threatened today by the indifference of their successors to its perversion. Today, as in Galileo's time, scientists dare not publish their results. The founders of science had to contend with the people's fear of sorcery. Not very different today is the reaction of a considerable section of a misunderstanding and revolted public. A few days ago, on television, I heard it suggested that we emulate Herod by killing all scientists at birth, but this I think would be going too far.

From a humanitarian standpoint I still believe that science is capable, if properly applied, of advancing the *real* happiness of mankind. In this matter I don't say that scientists have any particular responsibility, for actually science in its inception had to force itself, so to speak, upon the world. But now that it has grown to the point where its discoveries threaten the very existence of man, the case is very different. Scientists cannot escape responsibility for the results of their work, nor should they dare, if they have joy in discovery or if they value their own lives, to take a passive attitude toward war or to leave the control of atomic energy to politicians.

Ever since war became spatially three-dimensional, politicians have been promising to control it by traditional methods—attempting by *diplomacy* to prevent *nationalism* from asserting its prerogatives by *military force*. For half a century now these three forces have been cooperating to bring about the present crisis. A practitioner of the scientific method cannot be content to adhere to custom until the bombs begin to drop. Because one method fails, will he refuse to try another?

I believe it's time to try a new experiment. Regarding diplomacy, Henry Kissinger says: "It is not an accident that the diplomatic stalemate has become more intractable as weapons have grown more destructive . . . the increasing horror of war has made the progress of negotiations more difficult." Jerome Wiesner in *Daedalus* specifically states the difficulties and dangers of diplomatic disarmament conferences. Shall we trust our existence to such methods much longer? (Let us hope that the two conferences now arranged will produce results; nevertheless, let us be prepared for failure.)

Long experience of human affairs tells us that in all complex attempts at accomplishment there comes a time when discussion must cease and action must begin. Because of this experience all organizations have executives, one of whose qualifications must be courage to act. In the past, executive intervention in international affairs has often meant the start of a war. But since we

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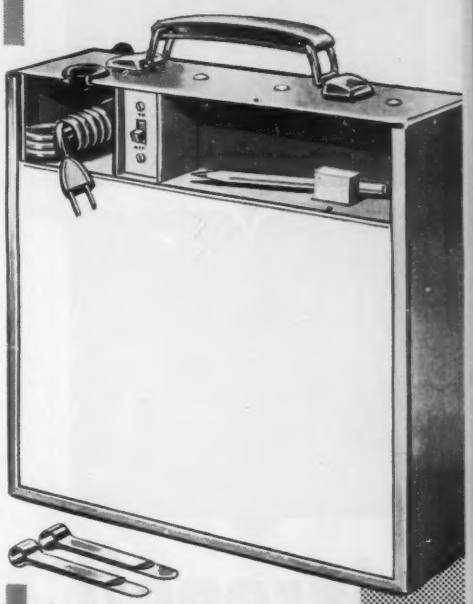
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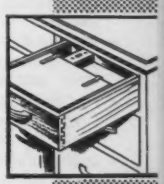
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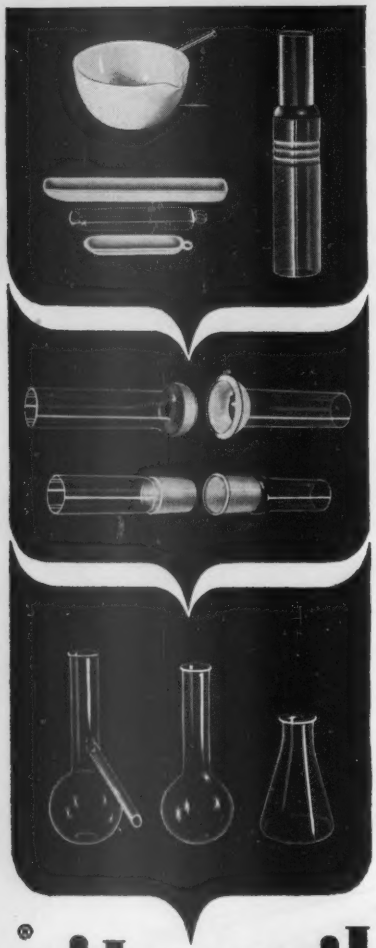

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are assured that war today is "unthinkable," let us now think how our President could initiate the attainment of world peace.

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Doubtless in implementing this process we would make some mistakes, but we would not be making the supreme mistake of talking until it is too late.

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On Educating the Public

Perhaps I may add something useful to James E. McDonald's letter [*Science* **133**, 1271 (21 Apr. 1961)]. Stung by repeated references to the callousness and indifference of scientists in the face of a threat of all-out nuclear war, and alarmed by the misinformation published by people who might be expected to know better, we at this university undertook a twofold project to educate the public. The first part of the project consisted of a series of 26 lectures on radiation and fallout, given in the extension department at a popular level. We are fortunate, being situated in Ottawa, which is the scientific as well as the political capital of Canada, in being able to call on experts from many different fields, ranging from the physicists and biologists at Atomic Energy of Canada, Limited (Chalk River), to the strategical and tactical experts associated with the armed forces. The second part of the project consisted of a series of dispassionate half-hour programs on television in which as much information

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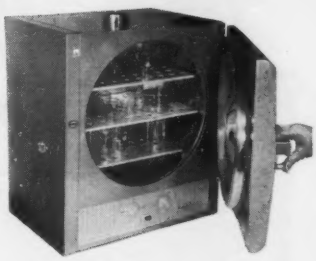

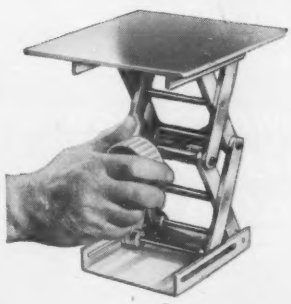

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as could usefully be presented through this medium was provided.

As a result of these two projects I have come to the following conclusions. First, and most disturbing, there is a very strong tendency among scientists who are specialists in the field of radiation damage to imagine that other scientists who are not specialists in the field are well informed upon the subject. This is certainly not the case, and in the preparation of our television programs we have had to wade through a plethora of material, usually confusing and badly presented, in order to obtain some sort of rational picture suitable for presentation to our colleagues and the general public. Some of the facts are certainly well summarized in McDonald's letter, but I have no confidence that more than a very small proportion of the scientific fraternity really understands the significance of the problem.

Second, it is quite obvious that the public as a whole does not wish to be informed upon this subject. In my naiveté I confidently expected that for such an exciting series of lectures, given by national experts, we should have a full lecture theatre. In fact, the number of registrations in this city of 250,000 inhabitants was fewer than 100; the attendance fell markedly as the series progressed, and this was no reflection on the lecturers, some of whom gave really outstanding presentations. As far as our television audience is concerned, we have reason to believe it is negligible; it is, of course, impossible for a person to judge his own performance, but we believe that our presentation has not been inadequate. The program is put on at a time of day when TV viewing is at a minimum (12 noon on Sunday), and this in itself is perhaps some commentary on the value placed on educational TV.

Third, it is rather easy to write a striking novel or film script on the subject of a global catastrophe, but it is not so easy to write one on the basis of our present knowledge of fallout. There was some beauty in *On the Beach*, but it is difficult to imagine a novel based on life during a nuclear attack that would be anything but sordid and depressing. I think this is the reason such a novel has not been written.

There is no doubt that the public has many misconceptions. To choose an example at random, despite the care that we took to deal accurately with the problem of radioactive dust in the lungs, it has been inferred by some of our audience that in minimizing this hazard we were, in fact, not quite telling the truth. I am afraid that the plain fact is that the problem is seen by the public in terms of black and white, whereas it is, of course, in tones of the subtlest gray. I see no hope whatever of reaching an understanding with the general public on this or any other aspect of modern military technology, and I think that perhaps we educators are wasting our time in thinking that such education is possible.

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An examination of the cited reference by the Army Medical Center group

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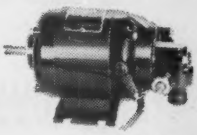

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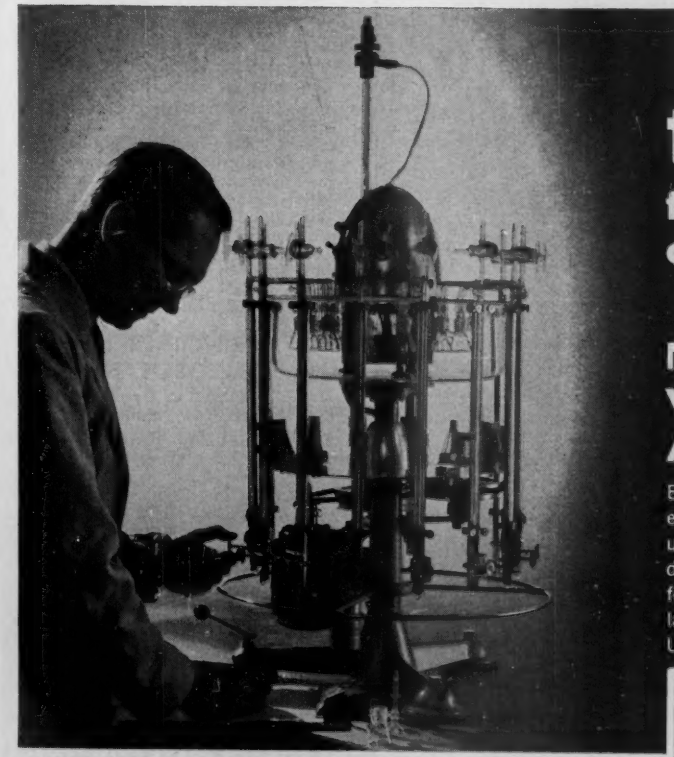

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(*2*) shows no mention of liver degeneration resulting from tetracyclines. On the contrary, this reference states that "one might consider the possibility" that chlortetracycline rather selectively depresses liver function, "but there is nothing to suggest such a possibility." The reference relates to a decline in the excretion of urobilinogen caused by changes in the intestinal flora resulting from the administration of chlortetracycline, and the authors support the concept that urobilinogen is formed by the action of intestinal bacteria on urobilin.

In other studies at the Army Medical Center (*3*), no significant changes in liver function resulted from the administration of chlortetracycline to rats, dogs, and human subjects, and other investigators have reported the alleviation of dietary hepatic necrosis in rats by this antibiotic (*4*). The various findings were reviewed by Hines (*5*).

THOMAS H. JUKES
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Reference (*10*) in our report should have been to an article by V. M. Sborov and D. A. Sutherland, "Fatty liver following aureomycin and terramycin therapy in chronic hepatic disease" [*Gastroenterology* **18**, 598 (1951)] instead of the article cited. These workers observed a temporary change in liver fat after Aureomycin and Terramycin therapy in chronic hepatic disease, and they compared these liver changes with other side effects reported in the literature to follow administration of tetracyclines.

Reference might also have been made to Yesner and Kunkel (*1*) or to Lepper *et al.* (*2*). The latter reported that, after administration of large intravenous doses to a number of patients, in addition to oral doses, in liver sections "there was much small vacuolization of the cytoplasm, with irregular fragmentation." In such cases the microscopic observation of changes in mitochondria in living liver cells by the technique described by us might supply further information regarding the nature of the changes leading to

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fatty metamorphosis (reversible fatty degeneration or, preferably, infiltration) of the liver. Regarding the nomenclature used, see (*3*). We, of course, did not intend to imply that in tetracycline therapy, as presently used, permanent liver damage will occur.

H. G. DU BUY
J. L. SHOWACRE
National Institute of Allergy and Infectious Diseases, Bethesda, Maryland

References

1. R. Yesner and P. Kunkel, *Yale J. Biol. Med.* **23**, 299 (1951).
2. M. H. Lepper, C. K. Wolfe, H. J. Zimmerman, E. R. Caldwell, H. W. Spies, H. F. Dowling, *A.M.A. Arch. Internal Med.* **88**, 271 (1951).
3. W. Boyd, *The Pathology of Internal Diseases* (Lea and Febiger, Philadelphia, rev. ed., 1940), p. 339.

Anonymous Reviewers

The letter by R. F. Shaw in *Science* [**133**, 1275 (1960)] impels me to raise another issue connected with the publication of scientific manuscripts—namely, the anonymous reviewer system. The anonymous reviewer system does well enough nine times out of ten, but it is intrinsically objectionable. What editor would pay attention to an anonymous communication? Why should the author of a scientific manuscript submit to such anonymous communications? Indeed, why should any objective reviewer hesitate to put his name to his opinion? Are not scientific book reviews signed?

SAMUEL RAYMOND
Pepper Laboratory of Clinical Medicine, University of Pennsylvania,

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Although many chemicals have been shown to have toxic effects when tested in animals, their use as food additives is permitted at specified levels and for specified purposes. This is because the severity and incidence of the toxic effects are related to the dose, a "no-effect" level being demonstrable.

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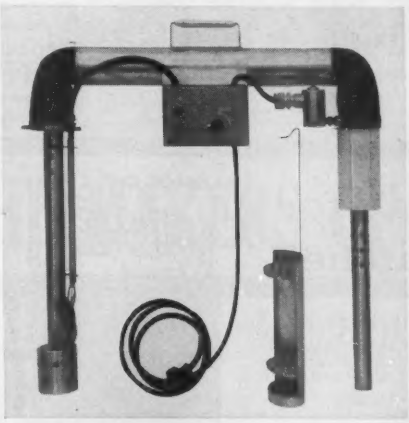

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It is perhaps correct and certainly prudent at this time to assume that there is no threshold dose as far as "primary" carcinogens are concerned. However, there are a number of "secondary" carcinogens, the carcinogenic activity of which is consequential to some other effects of these chemicals. For example, a substance when administered in large quantities may precipitate out from the urine and form bladder stone. The bladder stone in turn irritates the bladder, and as a result malignant tumor may develop.

A pharmacological action may also be the cause of cancer. For example, substance *X* inhibits the formation, release, or activity of thyroxin. This inhibition, among other things, raises the blood level of thyrotropic hormone. Malignant tumors in the thyroid may then develop, as a result of excessive stimulation by the thyrotropic hormone and possibly other factors. Since the solubility of a substance in the urine and the thyroxin-depressant action as well as other, similar pharmacological actions are readily definable and a "no-effect" level can be established, it appears that a noncarcinogenic dose exists in these cases. In other words, a safe dose for use of these chemicals as food additives may be worked out by the procedure adopted for chemicals possessing other types of toxicological action.

It may be argued that this proposal is not helpful because the mode of action of most carcinogens is unknown. However, if this distinction were more generally recognized, there would be incentive for scientists to carry out further research along this line.

F. C. Lu

Food and Drug Directorate, Department of National Health and Welfare, Ottawa, Canada

Indexing

Taken in the context of mechanical handling of information, the point raised by John R. Clark (*1*) has important implications. Given a lexicon of those words not to be indexed, electronic computers can create concordances from any portion of a paper, from the title up to the full text. A rapid, relatively inexpensive method of preparing a concordance is to make use of the words contained in the title of a paper. The keyword-in-context index, which was first proposed by Luhn (*2*) and which is now exemplified by the *Chemical Titles* current awareness service of the American Chemical Society, illustrates the use of titles in this manner.

Regarded analytically, a title may therefore be considered to be a collection of terms that help a reader to classify the paper in his mind, together with a set of connectives to show the relation of the terms to one another. Thus, if titles are to be used for machine preparation of concordances, the burden of providing suitable terms is in the hands of the author and the journal editor. Provision of suitable terms should be, not a "slight extra burden," but the key consideration in titling a paper.

Authors might, for instance, write down the terms they consider most suggestive of the content of their papers, then supply the necessary connectives in order to form the title. Of course, if the author has prepared 50 new chemical compounds he cannot list all of them in the title. As a general rule, one might say that he should use the most specific generic term (or small group of generic terms) available to him.

In terms of a thorough subject index, Clark has made the problem appear to be overly simple. The author of a paper is an authority on the work he has done, but there are problems of semantics, synonymy, specificity versus generality of terms, and nomenclature with which the untrained author cannot be expected to cope. The amount of time necessary to train an indexer is not available to scientist-authors. In addition, an author would find it time-consuming to try to decide which terms would be of interest to scientists in fields other than his own. Fleischer and Hooker have illustrated this point with regard to abstracts (*3*).

"Suitable standard systems of indexing" seem to be a practical impossibility for authors where traditional subject indexes are concerned. Authors do have the opportunity to improve titles so as to make machine-prepared concordances more effective.

Robert R. Freeman

Chemical Abstracts Service, Ohio State University, Columbus

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2. H. P. Luhn, *Am. Document.* **11**, 288 (1960).
3. M. Fleischer and H. Hooker, *J. Chem. Educ.* **33**, 27 (1956).

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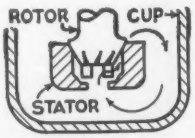

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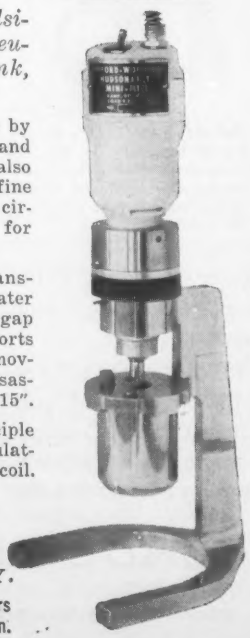

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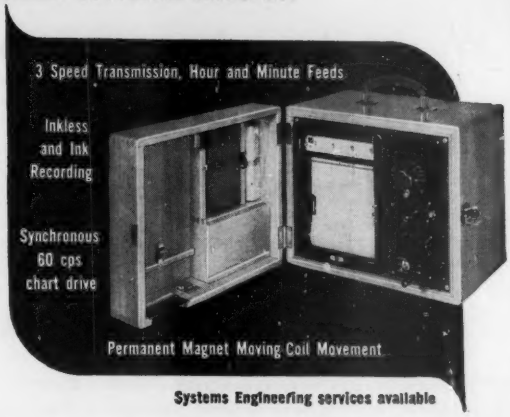
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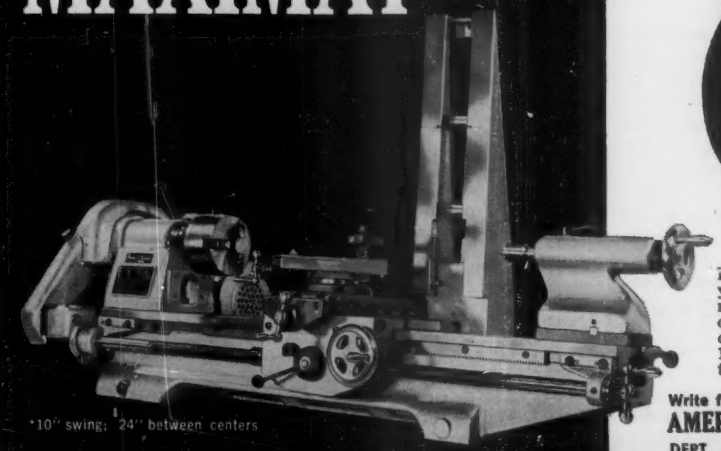

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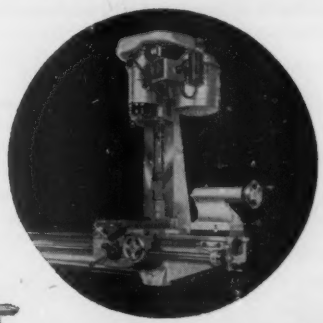

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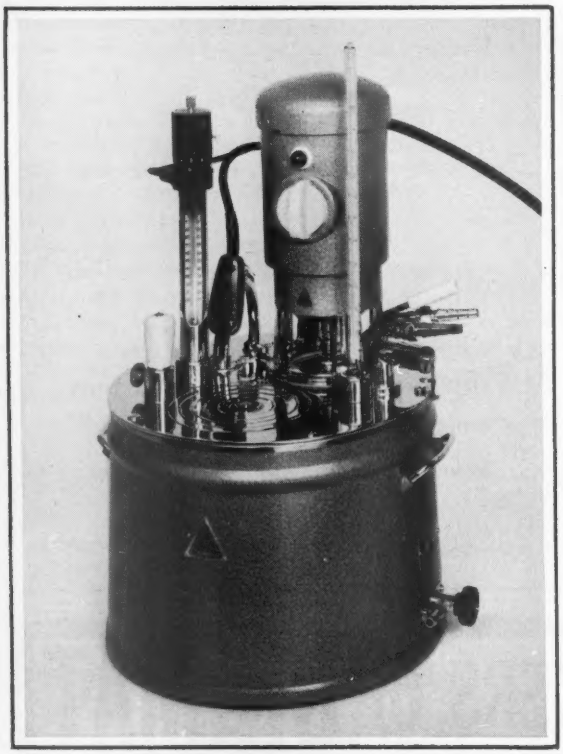

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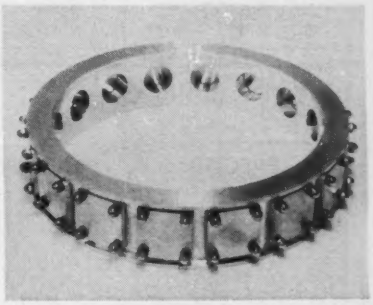

Fig. 1. Vacuum evaporator accessory.

Fig. 2. Stereoscopic microscope.

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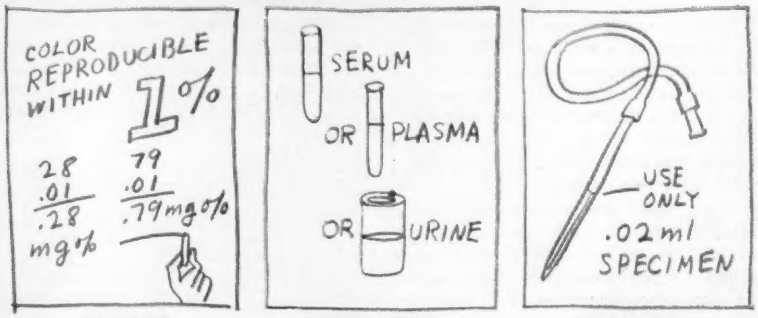

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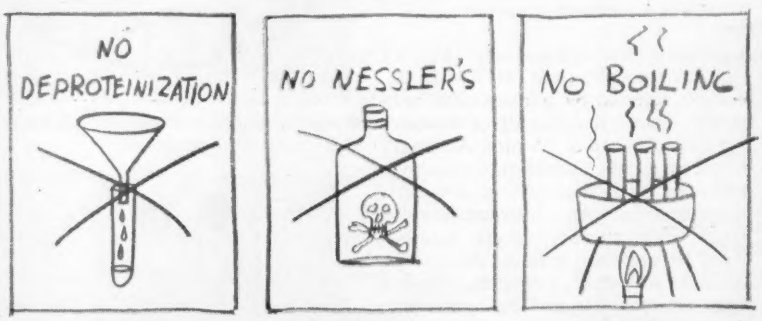

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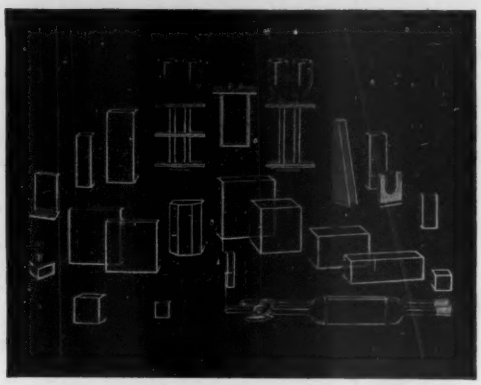

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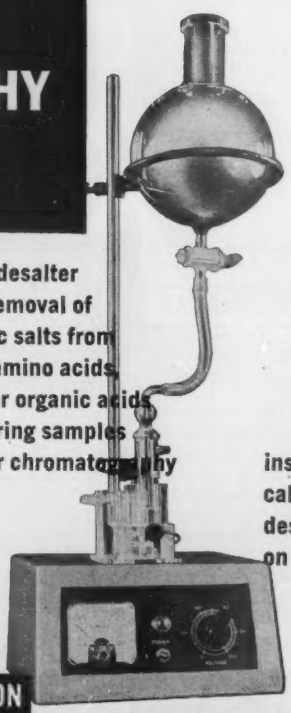

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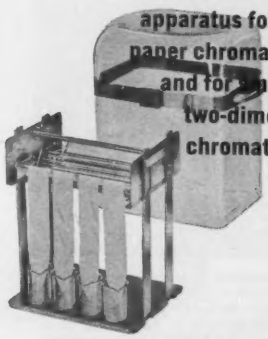

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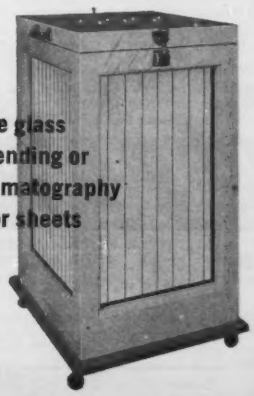

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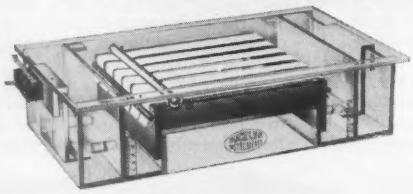

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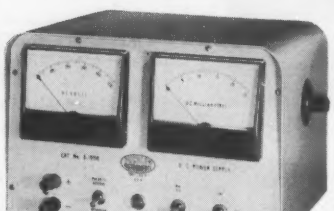

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■ INTEGRATING RECORDER for gas-chromatography applications integrates the area under each peak as the chromatogram is recorded. When the recorder pen leaves the zero line to start a peak, the integrator pen leaves its zero line and plots the cumulative area under the peak. When the integrator pen has traveled the full chart width, corresponding to 1000 counts, the pen automatically reverses. The integral reading is then the additional distance traveled by the pen in the return direction plus 1000. Integration counts of 10,000, 20,000, and 40,000 per minute are obtained by shifting gears. The integral curve can be started from either

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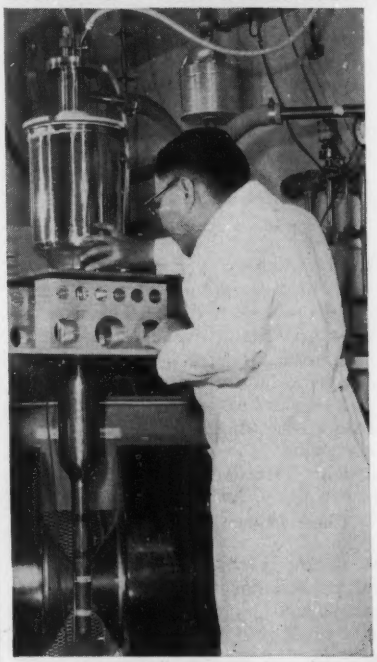

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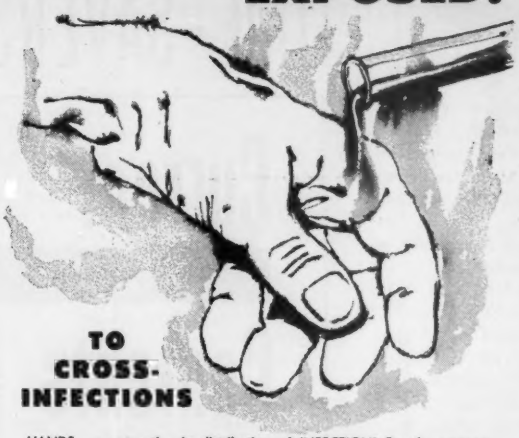

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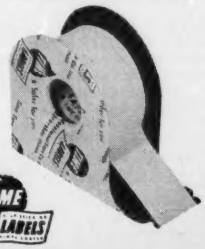

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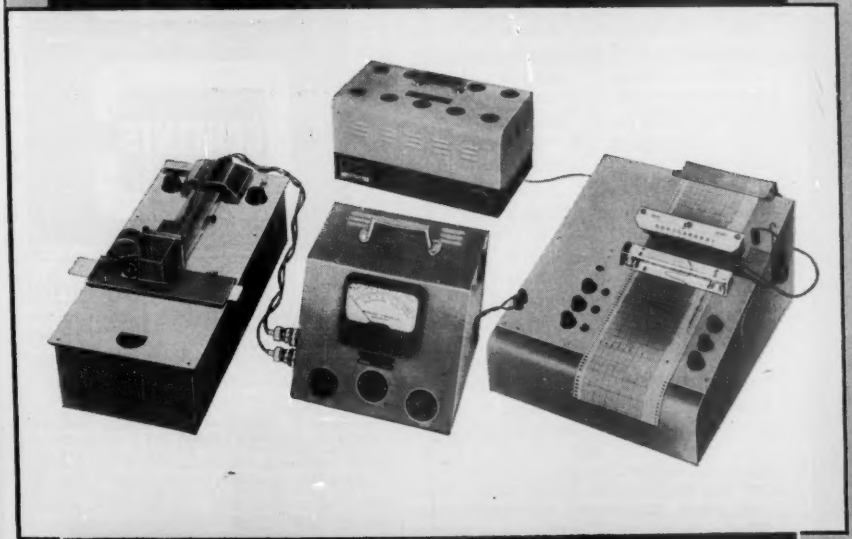

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Meetings

Plant Physiologists

On 6–8 February the southern section of the American Society of Plant Physiologists met with the Association of Southern Agricultural Workers at Jackson, Mississippi. All the sessions were well attended. The program included 28 contributed papers and a symposium on photosynthesis.

Chairman of the symposium was Hans Gaffron, of Florida State University. Papers were presented by André Jagendorf (McCollum-Pratt Institute, Johns Hopkins University), Birgit Vennesland (University of Chicago), and Roderick K. Clayton (Oak Ridge National Laboratory). The proceedings will be published, and copies will be available from the secretary-treasurer at a cost of $1.

The annual Plant Physiologists' breakfast was held on 8 February. An award of $25 for the best paper given by a graduate student was made to Theodore Holmsen (University of Florida) for a paper on geotropism in *Zea mays*. Award of a copy of volume 11 of the *Annual Review of Plant Physiology* was made to Coleman Ward (Virginia Polytechnic Institute) for his paper on effects of potassium levels on orchard grass and red clover in sand culture. The annual breakfast address was given by Harold Evans (North Carolina State College) on the essentiality of cobalt for leguminous plants grown under symbiotic conditions.

Officers elected for 1961 were as follows: chairman, Howard E. Joham (Texas Agricultural Experiment Station); vice-chairman, Robert D. Powell (University of Florida); and secretary-treasurer, Joseph C. O'Kelley (University of Alabama). Elected members of the executive committee are Howard Teas (University of Puerto Rico), Wayne C. Hall (Texas Agricultural Experiment Station), and Robert Burns (U.S. Department of Agriculture, Experiment, Ga.). Teas is the southern section representative to the American Society of Plant Physiologists.

JOSEPH C. O'KELLEY
University of Alabama, Tuscaloosa

Forthcoming Events

July

9–14. Bio-Medical Electronics, 4th intern. conf., New York, N.Y. (H. Schwan, Moore School of Electrical Engineering, University of Pennsylvania, Philadelphia)

9–15. American Library Assoc., annual conf., Cleveland, Ohio. (D. H. Clift, 50 E. Huron St., Chicago, Ill.)

9–15. International Dental Federation,

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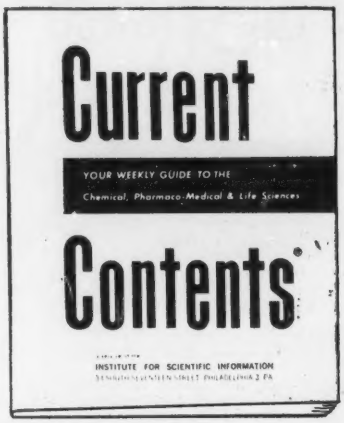

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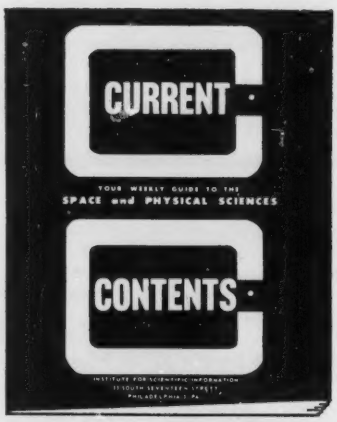

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49th annual session, Helsinki, Finland. (Office of Secretary General, IDF, 35 Devonshire Place, London, W.1, England)

10. Bibliographical Soc. of America, Cleveland, Ohio. (E. Wolf II, Library Co. of Philadelphia, Broad and Christian Sts., Philadelphia 47, Pa.)

10–14. Institute in Technical and Industrial Communications, 4th annual, Fort Collins, Colo. (Director, Inst. in Technical and Industrial Communications, Colorado State Univ., Fort Collins)

10–14. International Congr. of Dietetics, 3rd, London, England. (Miss D. F. Hollingsworth, British Dietetic Assoc. 251 Brampton Rd., London, S.W.3)

10–14. International Diabetes Federation, 4th congr., Geneva, Switzerland. (B. Rilliet, Secretary General, 4 Boulevard des Tranchees, Geneva)

10–14. Optical Instruments and Techniques, conf., London, England. (K. J. Habell, Natl. Physical Laboratory, Teddington, Middlesex, England)

10–20. Plant Exploration and Introduction, technical meeting on, Food and Agriculture Organization of the U.N., Rome, Italy. (Intern. Agency Liaison Branch, Office of the Director General, Viale della Terme di Caracalla, Rome)

11–25. World Meterological Organization, 3rd South American session, Rio de Janeiro, Brazil. (WMO, 1 Avenue de la Paix, Geneva, Switzerland)

12–18. Radioactivity in Food and Agriculture, Expert Committee on the Organization of Surveys for FAO, Rome, Italy. (Intern. Agency Liaison Branch, Office of the Director General, Viale della Terme di Caracalla, Rome)

13–14. Data Acquisition and Processing in Biology and Medicine, conf., Rochester, N.Y. (Office of Public Information, Univ. of Rochester, River Campus Station, Rochester 20)

15–18. Life Insurance Medicine, 7th intern. congr., Lisbon, Portugal. (L. de Carvalho Cancella, Secretary, Parede, Portugal)

16–18. British Congr. of Obstetrics and Gynaecology, 16th, Bristol, England. (Secretary, British Congr. of Obstetrics and Gynaecology, University Dept. of Obstetrics, Southmead Hospital, Bristol)

16–21. International Conf. on Medical Electronics, 4th, with Electrical Techniques in Medicine and Biology, 14th annual conf., New York, N.Y. (L. E. Flory, RCA Laboratories, Princeton, N.J.)

16–22. International Soc. for Clinical and Experimental Hypnosis, Rio de Janeiro, Brazil. (ISCEH, 33 E. 65 St., New York 21)

17–22. Soil Mechanics and Foundation Engineering, 5th intern. conf., Paris, France. (E. Caminade, Secretaire General, 23 rue de Cronstadt, Paris 15)

18–20. Pulmonary Structure and Function, Ciba Foundation Symp. (by invitation only), London, England. (Ciba Foundation, 41 Portland Pl., London, W.1)

18–21. Inorganic Polymers, intern. symp., Nottingham, England. (General Secretary, Chemical Soc., Burlington House, London, W.1, England)

21–22. World Power Conf. (members only), Moscow, U.S.S.R. (Central Office, 201-2 Grand Buildings, Trafalgar Sq., London, W.C.2, England)

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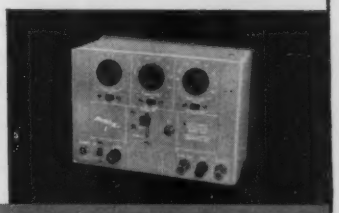

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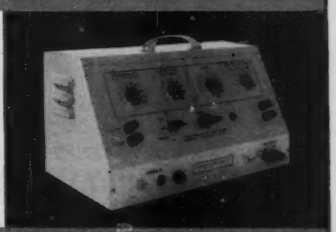

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23–28. Otolaryngology, 7th intern. congr., Paris, France. (H. Guillon, Secretary General, 6 Avenue Mac-Mahon, Paris 17)

24–28. Nematology Symp., 6th intern., Ghent, Belgium. (J. van den Brande, Soc. of European Nematologists, Rijkslandboushogeschool, Coupure links 235, Ghent)

24–29. Medical Electro-Radiological Societies, Latin Federation of, 5th congr., Paris, France. (C. Proux, Secretary, 9 rue Daru, Paris 8)

24–30. Urology, 12th intern. congr., Rio de Janeiro, Brazil. (J. Silva de Assis, Secretary, P.O. Box 1275, Belo-Horizonte, Brazil)

26. International Commission for the Prevention of Alcoholism, 7th annual meeting, Washington, D.C. (International Headquarters, 6840 Eastern Ave., NW, Washington 12)

26–28. Detection and Assay of Hormones by Immuno-Clinical Means, Ciba Foundation Colloquium (by invitation only), London, England. (Ciba Foundation, 41 Portland Pl., London, W.1)

27–1. Macromolecular Chemistry, intern. symp., Montreal, Canada. (Organizing Committee, P.O. Box 816, Sarnia, Ontario, Canada)

28–29. Linguistic Soc. of America, Austin, Tex. (A. A. Hill, Box 7790, University Station, Austin 12)

30–2. Soil Conservation Soc. of America, Lafayette, Ind. (H. W. Pritchard, 838 Fifth Ave., Des Moines 14, Iowa)

30–3. International Psycholanalytical Congr., 22nd, Edinburgh, Scotland. (Miss C. de Monehaux, 53 York Terrace, Regents Park, London, N.W.1, England)

31–4. Biophysics, 1st intern. congr., Stockholm, Sweden. (B. Lindström, Dept. of Medical Physics, Karolinska Institutet, Stockholm 60)

31–4. Differential Equations in Non-Linear Mechanics, Air Force Acad., Colorado Springs, Colo. (J. P. Lasalle, 7212 Bellona Ave., Baltimore 12, Md.)

31–11. Physics of the Solar System and Re-entry Dynamics, conf., Blacksburg, Va. (Bureau of Public Relations, Virginia Polytechnic Inst., Blacksburg)

31–12. Electric Power and Problems of Nuclear Power, seminar, U.N. Economic Commission for Latin America, Mexico, D.F. (A. Dorfman, Chief, Energy and Water Resource Program, Avenue Providencia 871, Santiago, Chile)

August

1–26. Functional Analysis, 8th American Mathematical Soc. summer institute, Stanford, Calif. (P. D. Lax, AMS, 190 Hope St., Providence 6, R.I.)

2–5. International Conf. of Pure and Applied Chemistry, 21st, Montreal, Canada. (R. Morf, Hoffmann-LaRoche, S.A., Grenzachterstrasse 124, Basel, Switzerland)

3–5. Canadian Chemical Conf. and Exhibition, 44th, Montreal. (Chemical Inst. of Canada, 48 Rideau St., Ottawa 2, Ont.)

5–9. International Rorschach Soc., 5th congr., Fribourg-en-Brisgau, Germany. (A. Friedemann, Chemin des Pêcheurs 6, Bienne, Switzerland)

6–10. Occupational Medicine and Toxicology, 3rd Inter-American conf., Miami.

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Fla. (W. B. Deichmann, School of Medicine, Univ. of Miami, Coral Gables, Fla.)

6–12. Atmospheric Ozone and General Circulation, symp., Arosa, Switzerland. (H. U. Duetsch, 20 Carl Spittelerstrasse, Zürich 53, Switzerland)

6–12. Chemical and Thermodynamic Properties at High Temperatures, symp., Montreal, Canada. (N. F. H. Bright, Natl. Research Council, Ottawa, Canada)

6–12. International Congr. of Pure and Applied Chemistry, 18th, Montreal, Canada. (L. Marion, Natl. Research Council, Ottawa 2, Canada)

7–9. Guidance and Navigation Conf., American Rocket Soc., Palo Alto, Calif. (J. J. Harford, ARS, 500 Fifth Ave., New York, N.Y.)

7–9. International Committee of Electro-Chemical Thermodynamics and Kinetics, 13th meeting, Montreal, Canada. (N. Ibl, Eidg. Technische Hochschule, Laboratorium für Physikalische und Elektrochemie, Universitätsstrasse 6, Zürich 6, Switzerland)

7–9. Space Age Astronomy, intern. symp., Pasadena, Calif. (D. W. Douglas, Jr., Douglas Aircraft Co., Inc., Santa Monica, Calif.)

7–10. National Medical Assoc., New York, N.Y. (J. T. Givens, 1108 Church St., Norfolk, Va.)

7–11. High Temperature Chemistry and Thermodynamics, symp., Montreal, Canada. (L. Brewer, Dept. of Chemistry, Univ. of California, Berkeley)

7–11. Seminar on Fast and Intermediate Reactors, International Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

8–11. Poultry Science Assoc., State College, Pa. (C. B. Ryan, Texas A & M College, College Station)

8–16. Society of Protozoologists, Prague, Czechoslovakia. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana)

10–16. International Congr. of Biochemistry, 5th, Moscow, U.S.S.R. (N. M. Sissakian, Leninsky prospekt, 33, Moscow, B-71)

10–16. International Union of Biochemistry, 4th general assembly, Moscow, U.S.S.R. (R. H. S. Thompson, IUB, Dept. of Chemical Pathology, Guy's Hospital Medical School, London, S.E.1, England)

12–19. Fast Reactions, summer school, Cambridge, England. (Secretary of the Summer School, Dept. of Physical Chemistry, Lensfield Road, Cambridge)

13–18. Microchemical Techniques, intern. symp., University Park, Pa. (H. J. Francis, Jr., Pennsalt Chemical Corp., P.O. Box 4388, Chestnut Hill Post Office, Philadelphia 18, Pa.)

13–18. Theoretical Aspects of Magnetohydrodynamics, seminar, University Park, Pa. (Conference Center, Pennsylvania State Univ., University Park)

13–19. International Assoc. of Applied Psychology, 14th congr., Copenhagen, Denmark. (Congress Secretariat, 19 Sankt Pederstraede, Copenhagen K.)

13–19. Training for Research in the Processes of Vision, 1st intern. conf., Rochester, N.Y. (Office of Public Information, River Campus Station, Rochester)

14–17. Calorimetry Conf., intern., Ottawa, Canada. (J. E. Kunzler, Bell Telephone Laboratories, Murray Hill, N.J.)

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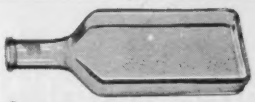

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14–19. International Medical Conf. on Mental Retardation, 2nd, Vienna, Austria. (Miss E. Langer, Div. of Maternal and Child Health, State House, Augusta, Maine)

14–19. Symposium on Radiation, Vienna, Austria. (World Meteorological Organization, 1 Avenue de la Paix, Geneva, Switzerland)

14–25. Israel Medical Assoc., 5th world assembly, Jerusalem, Israel. (Beth-Harofeh, 1 Heffman St., Tel-Aviv, Israel)

14–26. Plant Pathology, conf., Lafayette, Ind. (J. F. Schafer, Dept. of Botany and Plant Pathology, Purdue Univ., Lafayette)

14–26. World Eucalyptus Conf., 2nd, São Paulo, Brazil. (Intern. Agency Liaison Branch, Office of the Director General, Food and Agriculture Organization, Viale delle Terme di Caracalla, Rome, Italy)

15–17. International Assoc. of Milk and Food Sanitarians, Jekyll Island, Ga. (H. L. Thomasson, P.O. Box 437, Shelbyville, Ind.)

15–18. Technical Assoc. of the Pulp and Paper Industry, 12th testing conf., Montreal, Canada. (TAPPI, 155 E. 44 St., New York 16)

15–24. International Astronomical Union, 11th general assembly, Berkeley, Calif. (D. H. Sadler, Royal Greenwich Observatory, Herstmonceux Castle, Hailsham, Sussex, England)

16–18. Hypersonics Conf., intern., Cambridge, Mass. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

18–21. Association of American Geographers, East Lansing, Mich. (M. F. Burrill, 1785 Massachusetts Ave., NW, Washington 6)

19–30. Agricultural Economists, 11th intern. conf., Cuernavaca, Mexico. (J. Ackerman, Farm Foundation, 600 S. Michigan Ave., Chicago, Ill.)

20–23. International Ergonomics Assoc., 1st congr., Stockholm, Sweden. (T. Olson, Dept. of Industrial Physiology, G.C.I. Lidingövägen 1, Stockholm)

20–24. American Veterinary Medical Assoc., Detroit, Mich. (H. E. Kingman, AVMA, 600 S. Michigan Ave., Chicago 5, Ill.)

21–23. International Hypersonics Conf., Cambridge, Mass. (F. Ridell, Avco Research Laboratory, 301 Lowell St., Wilmington, Mass.)

21–24. Biological Photographic Assoc., Chicago, Ill. (Mrs. J. W. Crouch, Box 1668, Grand Central P.O., New York 17)

21–24. International Conf. on Photoconductivity, Ithaca, N.Y. (E. Burstein, Dept. of Physics, Univ. of Pennsylvania, Philadelphia)

21–26. International Congr. of Psychotherapy, 5th, Vienna, Austria. (W. Spiel, Lazarettg. 14, Vienna 9)

21–26. World Traffic Engineering Conf., Washington, D.C. (Intern. Road Federation, 1023 Washington Bldg., Washington 5)

21–27. International Assoc. of Dental Students, congr., London, England. (D. H. Clark, Royal Dental Hospital, Leicester Sq., London, W.C.2)

21–31. United Nations Conf. on New Sources of Energy, Rome, Italy. (United Nations, New York, N.Y.)

21–2. International Congr. of Practical Medicine, Merano, Italy. (Bundesärztekammer, 1 Hädenkampfstrasse, Cologne, Germany)

21–6. Pacific Science Congr., 10th, Honolulu, Hawaii. (Secretary General, 10th Pacific Science Congr., Bishop Museum, Honolulu)

22–25. International Pharmacological Meeting, 1st, Stockholm, Sweden. (A. Wretlind, Karolinska Institutet, Stockholm 60)

22–30. International Conf. on Protozoology, Prague, Czechoslovakia. (N. D. Levine, College of Veterinary Medicine, Univ. of Illinois, Urbana)

23–25. Gas Dynamics, symp., biennial, Evanston, Ill. (J. J. Harford, American Rocket Soc., 500 Fifth Ave., New York, N.Y.)

23–26. Electron Microscope Soc. of America, Pittsburgh, Pa. (Miss M. L. Rollins, Agricultural Research Service, U.S. Department of Agriculture, P.O. Box 19,687, New Orleans 19, La.)

23–26. Institute of Management Sciences, 8th annual intern., Brussels, Belgium. (W. Smith, Inst. of Science and Technology, Univ. of Michigan, Ann Arbor)

23–1. Radioisotopes in the Biological Sciences, conf., Intern. Atomic Energy Agency, Vienna, Austria. (IAEA, 11 Kärtner Ring, Vienna 1)

24–26. Physiology of the Hippocampus, intern. colloquium, Montpellier, France. (Mme. Mineur, Centre National de la Recherche Scientifique, 13 Quai Anatole France, Paris 7)

26–1. Radiology, 10th intern. congr., Montreal, Canada. (C. B. Peirce, Suite 204, 1555 Summerhill, Montreal 25)

26–2. History of Science, 5th intern.

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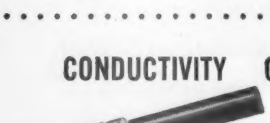

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27–29. International Congr. of Group Psychotherapy, 3rd, Paris, France. (W. Warner, P.O. Box 819, Grand Central Station, New York 17)

27–29. Psychosomatic Aspects of Neoplastic Disease, 2nd annual conv., Paris, France. (L. L. LeShan, Intern. Psychosomatic Cancer Study Group, 144 E. 90 St., New York 28)

27–31. American Soc. of Plant Psysiologists, Lafayette, Ind. (C. O. Miller, Indiana Univ., Bloomington)

27–1. American Congr. of Physical Medicine and Rehabilitation, Cleveland, Ohio. (D. C. Augustin, 30 N. Michigan Ave., Chicago 2, Ill.)

27–1. American Inst. of Biological Sciences, annual, Lafayette, Ind. (J. R. Olive, AIBS, 2000 P St., NW, Washington 6)

27–1. Coordination Chemistry, 6th intern. conf., Detroit, Mich. (S. Kirschner, Dept. of Chemistry, Wayne State Univ., Detroit 2)

28–30. Mathematical Assoc. of America, Stillwater, Okla. (H. L. Alder, MAA, Univ. of California, Davis)

28–30. Oak Ridge Inst. of Nuclear Studies, 8th annual summer symp., Gatlinburg, Tenn. (Symposium Office, University Relations Division, Oak Ridge Inst. of Nuclear Studies, P.O. Box 117, Oak Ridge, Tenn.)

28–30. Scandinavian Symp. on Fat Rancidity, 3rd, Sandefjord, Norway. (E. Törnudd, Gaustadallen 30, Blindern, Norway)

28–31. American Assoc. of Clinical Chemists, natl., New York, N.Y. (B. Klein, Chemistry Dept., Kingsbridge V.A. Hospital, Bronx, N.Y.)

28–31, American Soc. for Pharmacology and Experimental Therapeutics, Rochester, N.Y. (K. H. Beyer, Merck, Sharp and Dohme Research Laboratories, West Point, Pa.)

28–31. Botanical Soc. of America, Lafayette, Ind. (B. L. Turner, Dept. of Botany, Univ. of Texas, Austin 12)

28–31. Chemical Physics of Nonmetallic Crystals, intern. conf., Evanston, Ill. (O. C. Simpson, Argonne National Laboratory, 9700 South Cass Ave., Argonne, Ill.)

28–1. Heat Transfer Conf., intern., Boulder, Colo. (S. P. Kezios, American Soc. of Mechanical Engineers, 29 W. 39 St., New York 18)

28–1. Ionization Phenomena in Gases, 5th intern. conf., Munich, Germany. (Secretariat, Oskar von Miller Ring 18, P.O. 463, Munich 1)

28–1. Radioactive Metrology, symp., Oxford, England. (B. W. Robinson, Applied Physics Division, National Physical Laboratory, Teddington, Middlesex, England)

28–1. Rockets and Astronautics, 3rd intern. symp., Tokyo, Japan. (Japanese Rocket Soc., 1-3, Ginza-Nishi, Chuo-Ku, Tokyo)

28–2. European Soc. of Haematology, 8th congr., Vienna, Austria. (H. Fleischhracker, Frankgasse 8, Billrothhaus, Vienna 9)

28–2. International Assoc. of Medical

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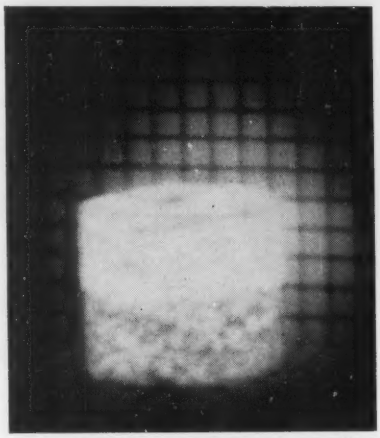

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28–2. Detonation Waves, intern. colloquium, Gif-sur-Yvette, France. (G. M. Ribaud, Centre National de la Recherche Scientifique, 13 Quai Anatole France, Paris 7, France)

28–2. Mechanics of Turbulence, intern. colloquium, Marseilles, France. (A. Favre, Faculté des Sciences, Université, Marseilles)

29. American Soc. for Horticultural Science, Lafayette, Ind. (R. E. Marshall, Dept. of Horticulture, Michigan State Univ., East Lansing)

29–1. American Mathematical Soc., 66th summer meeting and 40th colloquium, Stillwater, Okla. (J. W. T. Youngs, AMS, 190 Hope St., Providence 6, R.I.)

29–1. Society for Industrial and Applied Mathematics, Stillwater, Okla. (G. Kaskey, Remington Rand UNIVAC, P.O. Box 500, Blue Bell, Pa.)

29–6. Planning of Experiments, intern. colloquium, Paris, France. (D. Dugue, Institut de Statistique de l'Université, 11 rue Pierre Curie, Paris 5)

29–7. International Statistical Inst., 33rd session, Paris, France. (G. R. Chevry, 29 Quai Branly, Paris 7)

30–1. Semiconductor Conf., 3rd annual, Los Angeles, Calif. (W. V. Wright, Electro-Optical Systems, Inc., 125 N. Vinedo Ave., Pasadena, Calif.)

30–2. American Sociological Assoc., St. Louis, Mo. (T. Parsons, Dept. of Social Relations, Harvard Univ., Cambridge, Mass.)

30–2. Experimental Research on Shell Structures, colloquium, Delft, Netherlands. (A. L. Bouma, Dept. of Civil Engineering, Technological Univ., Delft)

30–5. Mental Health, 6th intern. congr., Paris, France. (Miss E. M. Thornton, World Federation for Mental Health, 19 Manchester St., London, W.1, England)

31–2. Exfoliative Cytology, intern. congr., Vienna, Austria. (Office of the Secretary of the Congress, 666 Elm St., Buffalo 3, N.Y.)

31–2. Gynaecological Cytology, 1st intern. congr., Vienna, Austria. (R. M. Graham, Roswell Park Memorial Inst., 666 Elm St., Buffalo 3, N.Y.)

31–4. Preventive and Social Medicine, meeting, Evian, France (Societé Française de Medecine Preventive et Sociale, 1 rue de Courcelles, Paris 8, France)

31–6. American Psychological Assoc., 69th annual, New York, N.Y. (J. G. Darley, 1333 16th St., NW, Washington 6)

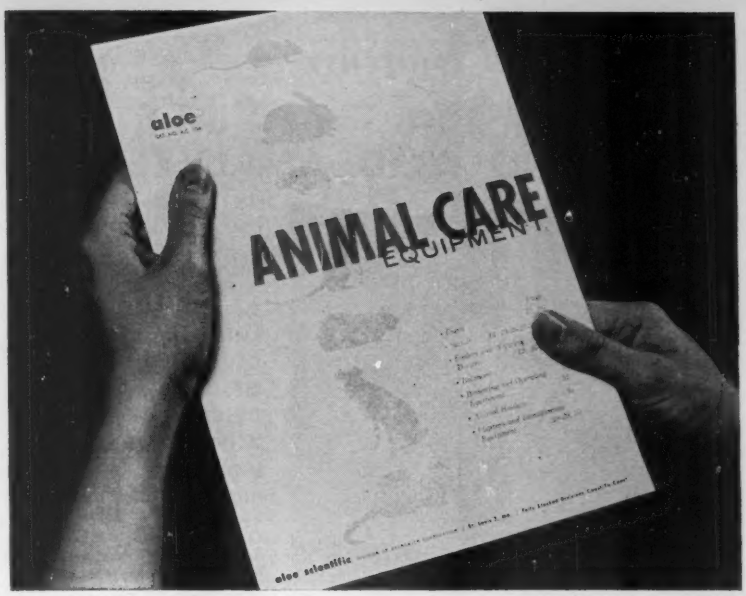
September

1–5. Danube Research, intern. symp., Budapest, Hungary. (Biological Sciences Group, Hungarian Acad. of Sciences, Roosevelt Tèr. 9, Budapest V)

1–9. Topology and Its Methods in Other Mathematical Disciplines, symp., Prague, Czechoslovakia. (Organizing Committee, Ke Karlovu 3, Prague 2)

1–10. International Pharmaceutical Students' Federation, 7th congr., Munich, Germany. (U. Peto, 10 Groffstr., Munich 19)

2–7. International Assoc. for Quater-



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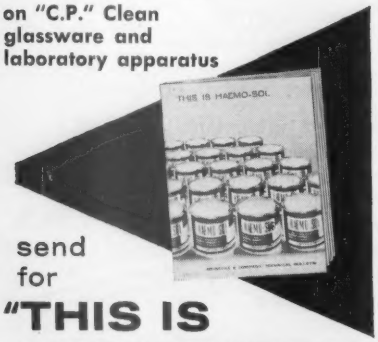

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nary Research, Warsaw, Poland. (R. Galon, Secretary General, INQUA, Geographical Inst. Univ., Torun, Poland)

2–9. International Soc. of Surgery, 19th congr., Dublin, Ireland. (T. C. J. O'Connell, 35 Fitzwilliam Pl., Dublin)

3–7. International Assoc. for Hydraulic Research, 9th congr., Belgrade, Yugoslavia. (H. J. Schoemaker, Waterloopkundig Laboratorium, Raam 61, Delft, Netherlands)

3–8. American Chemical Soc., 140th meeting, Chicago, Ill. (A. T. Windstead, National Meetings Dept., ACS, 1155 16 St., NW, Washington 6)

3–9. International Federation of Gynaecology and Obstetrics, 3rd world congr., Vienna, Austria. (V. Grünberger, Medizinische Akademie, Alserstrasse 4, Vienna 9)

3–10. Inter-American Congr. of Radiology, 7th, São Paulo, Brazil. (W. Bomfim-Pontes, Rua Cesario Motta 112, São Paulo)

4. World Federation for Mental Health. 14th annual, Paris, France. (WFMH, 19 Manchester St., London, W.1, England)

4–6. International Assoc. for Shell Structures, colloquium, Brussels, Belgium. (Prof. Dutron, 127 Avenue Adolphe Buyl, Brussels 5)

4–6. International Symp. on the Earth Storm, Kyoto, Japan. (T. Nagata, Science Council of Japan, Ueno Park, Tokyo)

4–7. Neuropathology, 4th intern. congr., Munich, Germany. (W. Haymaker, Armed Forces Inst. of Pathology, Walter Reed Army Medical Center, Washington 25)

4–7. Rheumatology, 10th intern. congr., Rome, Italy. (C. B. Ballabio, Clinica Medica Generale, Via F. Sforza 35, Milan, Italy)

4–8. Low Energy Nuclear Physics, intern. conf., Manchester, England. (L. J. B. Goldfarb, Physics Dept., Univ. of Manchester, Manchester)

4–8. Pharmaceutical Sciences, 21st intern., congr., Pisa, Italy. (Intern. Pharmaceutical Federation, 11 Alexanderstraat, The Hague, Netherlands)

4–8. Plasma Physics and Controlled Nuclear Fusion Research, conf., Salzburg, Austria. (Intern. Atomic Energy Agency, United Nations, New York, N.Y.)

4–9. International Assoc. for Analog Computation, 3rd intern. sessions, Belgrade, Yugoslavia. (D. Strujic, Decanska 14/IV, Belgrade)

4–9. International Congr. of Angiology, 4th, Prague, Czechoslovakia. (Z. Reinis, 4th Medical Clinic, Prague 2/499)

4–9. International Symp. on Fundamental Problems in Turbulence and Their Relation to Geophysics (by invitation), Marseilles, France. (Intern. Union of Geodesy and Geophysics, 53 Avenue de Breteuil, Paris 7)

4–9. Laurentian Hormone Conf., Hoberg's Resort, Lake County, Calif. (Committee on Arrangement of the Laurentian Hormone Conference, 222 Maple Ave., Shrewsbury, Mass.)

4–13. Inter-African Conf. for Food and Nutrition, 4th, Bukavu, Congo Republic. (Commission for Technical Cooperation in Africa South of the Sahara, Pvt. Mail Bag 2359, Lagos, Nigeria)

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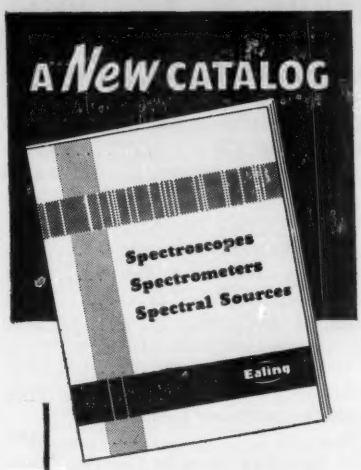

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5–8. International Congr. of Homeopathic Medicine, 25th, Amsterdam, Netherlands. (J. L. Fonteijn, Westzijde 116, Zaandam, Netherlands)

5–8. Machine Translation of Languages and Applied Language Analysis, intern. conf., Teddington, England. (L. Dostert, Director, Machine Translation Research, Georgetown Univ., 1715 Massachusetts Ave., NW, Washington 6)

5–8. National Chemical Exposition, 11th, Chicago, Ill. (Chicago Section, American Chemical Soc., 86 E. Randolph St., Chicago 1)

6–8. Effects of Ionizing Radiations on Immune Processes, intern. symp., Lawrence, Kan. (C. A. Leone, Dept. of Zoology, Univ. of Kansas, Lawrence)

6–8. Transmission and Processing of Information, intern. symp., Boston, Mass. (R. M. Fano, Research Laboratory of Electronics, Massachusetts Inst. of Technology, Cambridge 39)

6–12. Human Genetics, 2nd intern. conf., Rome, Italy. (L. Gedda, 5 Piazza Galeno, Rome)

7–8. Pacific Slope Biochemical Conf., annual, San Diego, Calif. (R. G. Wolfe, Chemistry Dept., Univ. of Oregon, Eugene)

7–9. International Cardiovascular Soc., 5th congr., Dublin, Ireland. (H. Haimovici, 715 Park Ave., New York 21)

7–9. Parapsychological Assoc., 4th annual congr., New York, N.Y. (W. A. Roll, Box 6116, College Station, Durham, N.C.)

7–11. European Orthodontic Soc., 37th congr., Bologna, Italy. (N. Gray, 16 College Rd., Eastbourne, Sussex, England)

7–12. Neurogenetics, symp., Rome, Italy. (L. Gedda, Instituto Gregorio Mendel, Viale Regina Margherita 261, Rome)

7–13. Electroencephalography and Clinical Neurophysiology, 5th intern. congr., Rome, Italy. (R. Vizioli, Viale Universita 30, Rome)

10–14. Tuberculosis Conf., 16th intern., Toronto, Canada. (C. W. L. Jeanes, 265 Elgin St., Ottawa, Ont., Canada)

10–15. Neurology, 7th intern. congr., Rome, Italy. (G. Alema, Viale Università 30, Rome)

10–17. International Union of Forest Research Organizations, 13th congr., Vienna, Austria. (Forest Research Inst., IUFRO Bureau, Vienna 89)

11–13. European Organization for Quality Control, 5th congr., Turin, Italy. (Weena 700, Rotterdam, Netherlands)

11–14. International Flax and Hemp Federation, 12th congr., Lisbon, Portugal. (IFHF, 37 rue de Courcelles, Paris 8)

11–15. Cosmic Rays, 7th intern. conf., Kyoto, Japan. (Y. Sekido, Science Council of Japan, Ueno Park, Tokyo)

11–15. Cybernetics, 3rd intern. congr., Namur, Belgium. (Intern. Assoc. for Cybernetics, 13 rue Basse-Marcelle, Namur)

11–15. Instrument Soc. of America, instrument-automation conf. and exhibit, 16th, Los Angeles, Calif. (W. H. Kushnick, 313 Sixth Ave., Pittsburgh 22, Pa.)

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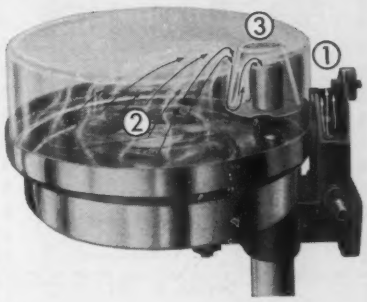

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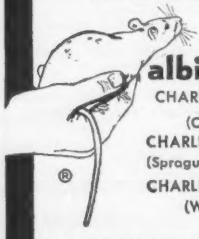

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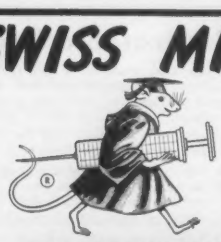

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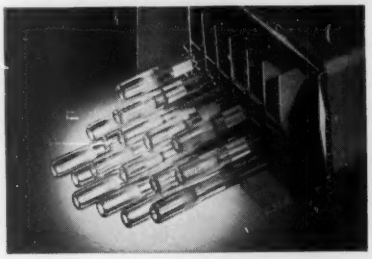

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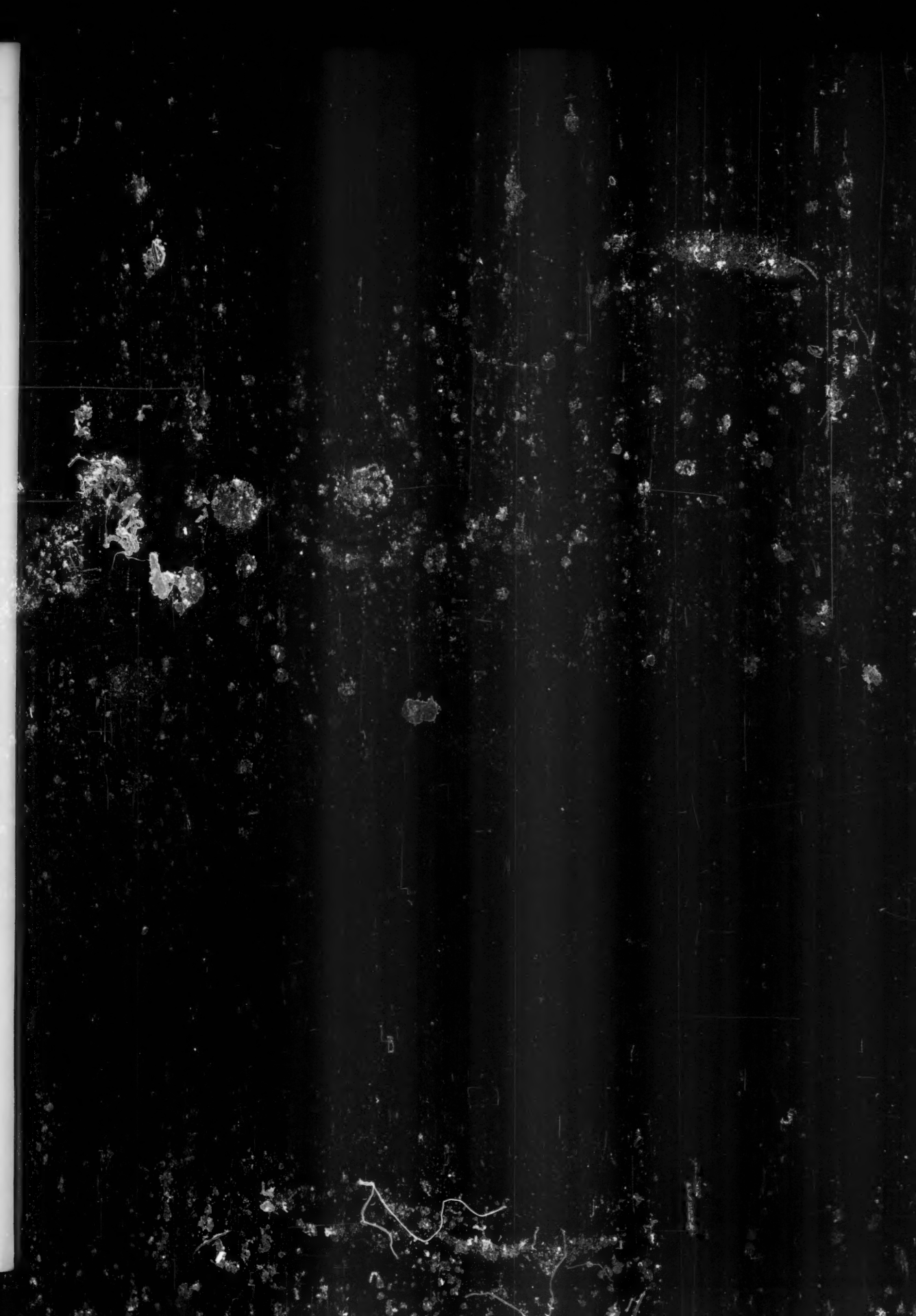
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